



This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

- + *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.
- + *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.
- + *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.
- + *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at <http://books.google.com/>



R. 819
P



LELAND STANFORD JUNIOR UNIVERSITY

[REDACTED]

G

PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From March 3, 1898, to June 16, 1898.

VOL. LXIII.

LONDON:

HARRISON AND SONS, ST. MARTIN'S LANE,

Printers in Ordinary to Her Majesty.

MDCCXCVIII.

LONDON:
HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY,
ST. MARTIN'S LANE.

112686

CONTENTS.

VOL. LXIII.



No. 389.

	Page
Meeting of March 3, 1898, and List of Candidates for Election	1
List of Papers read.....	2
On the Depletion of the Endosperm of <i>Hordeum vulgare</i> during Germination. By Horace T. Brown, F.R.S., and F. Escombe, B.Sc., F.L.S. (Plate 1)	3

No. 390.

Meeting of March 10, 1898, and List of Papers read	25
On the Modifications of the Spectra of Iron and other Substances radiating in a strong Magnetic Field. By Thomas Preston, M.A. Communicated by Professor Geo. Francis FitzGerald, F.R.S.....	26
Note on the Connection between the Faraday Rotation of Plane of Polarisation and the Zeeman Change of Frequency of Light Vibrations in a Magnetic Field. By Geo. Fras. FitzGerald, F.R.S., F.T.C.D.	31
On Artificial Temporary Colour-blindness, with an Examination of the Colour Sensations of 109 Persons. By George J. Burch, M.A. Communicated by Professor Gotch, F.R.S.	35
On the Connection between the Electrical Properties and the Chemical Composition of different kinds of Glass. By Professor Andrew Gray, LL.D., F.R.S., and Professor J. J. Dobbie, M.A., D.Sc.	38
On the Magnetic Deformation of Nickel. By E. Taylor Jones, D.Sc. Communicated by Professor Andrew Gray, F.R.S.	44
Upon the Structure and Development of the Enamel of Elasmobranch Fishes. By Charles S. Tomes, M.A., F.R.S.....	54
On Apogamy and the Development of Sporangia upon Fern Prothalli. By William H. Lang, M.B., B.Sc., Lecturer in Botany, Queen Margaret College, and "G. A. Clark" Scholar, Glasgow University. Communicated by Professor F. O. Bower, Sc.D., F.R.S.	56
Experimental Observations on the Early Degenerative Changes in the Sensory End Organs of Muscles. By F. E. Batten, M.D. Communicated by Professor Victor Horsley, F.R.S.....	61

No. 391.

	Page
Meeting of March 17, 1898, Croonian Lecture, &c.	63
Meeting of March 24, 1898, Bakerian Lecture, &c.	63
On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force and the Period of Solar Spot Frequency. By William Ellis, F.R.S., formerly of the Royal Observatory, Greenwich	64
On the Relative Retardation between the Components of a Stream of Light produced by the Passage of the Stream through a Crystalline Plate cut in any direction with respect to the Faces of the Crystal. By James Walker, M.A. Communicated by Professor R. B. Clifton, F.R.S.	79
An Extension of Maxwell's Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and Allied Phenomena. By Edwin Edser, A.R.C.S. Communicated by Captain W. de W. Abney, C.B., F.R.S.	91
CROONIAN LECTURE.—The Nature and Significance of Functional Metabolism in the Plant (<i>Das Wesen und die Bedeutung des Betriebsstoffwechsels in der Pflanze</i>). By Wilhelm Pfeffer, Sc.D. (Cantab.), of the University of Leipzig, For. Mem. R.S.	93
BAKERIAN LECTURE.—Further Experiments on the Action exerted by certain Metals and other Bodies on a Photographic Plate. By W. J. Russell, Ph.D., V.P.R.S.	102

No. 392.

On Contact Electricity of Metals. By J. Erskine-Murray, D.Sc., F.R.S.E., Heriot-Watt College. Communicated by Lord Kelvin, G.C.V.O., F.R.S.	113
--	-----

No. 393.

the Rotation of Plane of Polarisation of Electric Waves by a Twisted Structure. By Jagadis Chunder Bose, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord Rayleigh, F.R.S.	146
On the Production of a "Dark Cross" in the Field of Electro-magnetic Radiation. By Jagadis Chunder Bose, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord Rayleigh, F.R.S.	152
The Relations between Marine Animal and Vegetable Life. By H. M. Vernon, M.A., M.B. Communicated by Professor Burdon Sanderson, F.R.S.	155

No. 394.

Report of the Kew Observatory Committee for the Year ending December 31, 1897	161
---	-----

	Page
On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helical Current and a Uniform Coaxial Circular Cylindrical Current Sheet. By Professor J. Viriamu Jones, F.R.S.	192
Meeting of March 31, 1898	205

No. 395.

Meeting of April 28, 1898, and List of Papers read	206
On the Meteorological Observatories of the Azores. By H.S.H. Prince Albert I of Monaco. Communicated by J. Y. Buchanan, F.R.S.....	206
A Compensated Interference Dilatometer. By A. E. Tutton, Assoc. R.C.S. Communicated by Captain Abney, C.B., F.R.S.	208
Meeting of May 5, 1898, and List of Papers read	212
Observations on the Action of Anæsthetics on Vegetable and Animal Protoplasm. By J. B. Farmer, M.A., and A. D. Waller, M.D., F.R.S.	213
On certain Structures formed in the Drying of a Fluid with Particles in Suspension. By Catherine A. Raisin, B.Sc. Communicated by Professor T. G. Bonney, F.R.S. (Plate 2)	217
The Relations between the Hybrid and Parent Forms of Echinoid Larvæ. By H. M. Vernon, M.A., M.B. Communicated by Professor E. Ray Lankester, F.R.S.	228

No. 396.

Meeting of May 12, 1898, and List of Papers read.....	231
A Calorimeter for the Human Body. By William Marcet, M.D., F.R.S.	232
An Experimental Enquiry into the Heat given out by the Human Body. By W. Marcet, M.D., F.R.S., and R. B. Floris, F.C.S.....	242
Preliminary Note on the Liquefaction of Hydrogen and Helium. By James Dewar, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution.....	256
Effects of Prolonged Heating on the Magnetic Properties of Iron. By S. R. Roget, B.A. Communicated by Professor Ewing, F.R.S.....	258
On the Connection of Algebraic Functions with Automorphic Functions. By E. T. Whittaker, B.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. R. Forsyth, Sc.D., F.R.S.	267
A Study of the Phyto-Plankton of the Atlantic. By George Murray, F.R.S., Keeper of Botany, British Museum, and V. H. Blackman, B.A., F.L.S., Hutchinson Student, St. John's College, Cambridge, and Assistant, Department of Botany, British Museum....	269

No. 397.

	Page
Meeting of May 26, 1898, and List of Papers read.....	270
On the Intimate Structure of Crystals. Part I. Crystals of the Cubic System with Cubic Cleavage. By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford	270
On the Intimate Structure of Crystals. Part II. Crystals of the Cubic System with Cubic Cleavage. Haloid Compounds of Silver. By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford	286
On the Intimate Structure of Crystals. Part III. Crystals of the Cubic System with Cubic Cleavage. By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford.....	296
The Electrical Response of Nerve to a Single Stimulus investigated with the Capillary Electrometer. Preliminary Communication. By F. Gotch, M.A., F.R.S., Professor of Physiology, University of Oxford, and G. J. Burch, M.A. (Oxon.)	300

No. 398.

On the Magnetic Susceptibility of Liquid Oxygen. By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and James Dewar, M.A., LL.D., F.R.S., Fullerton Professor of Chemistry in the Royal Institution, London, &c.....	311
Aluminium as an Electrode in Cells for Direct and Alternate Currents. By E. Wilson. Communicated by Dr. J. Hopkinson, F.R.S.	329
Contributions to the Study of "Flicker." By T. C. Porter, Eton College. Communicated by Lord Rayleigh, F.R.S.	347
On the Kathode Fall of Potential in Gases. By J. W. Capstick, M.A., D.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. Thomson, F.R.S.	356
Note on the Complete Scheme of Electrodynamical Equations of a Moving Material Medium, and on Electrostriction. By Joseph Larmor, F.R.S., Fellow of St. John's College, Cambridge	365

No. 399.

Annual Meeting for the Election of Fellows	373
Meeting of June 9, 1898, and List of Papers read	373
An Extension of Maxwell's Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and Allied Phenomena. By Edwin Edser, A.R.C.S. Communicated by Captain W. de W. Abney, C.B., F.R.S.	374
A Photographic Investigation of the Absorption Spectra of Chlorophyll and its Derivatives in the Violet and Ultra-violet Region of the Spectrum. By C. A. Schunck. Communicated by Dr. E. Schunck, F.R.S. (Plates 3, 4, 5)	389

	Page
On Photographic Evidence of the Objective Reality of Combination Tones. By R. W. Forsyth, A.R.C.S., and R. J. Sowter, A.R.C.S. Communicated by Professor Rücker, Sec.R.S. (Plates 6, 7).....	396
On the Cytological Features of Fertilisation and Related Phenomena in <i>Pinus silvestris</i> , L. By Vernon H. Blackman, B.A., F.L.S., Hutchinson Student, St. John's College, Cambridge, and Assistant, Department of Botany, British Museum. Communicated by Francis Darwin, F.R.S.	400
Experiments on Aneroid Barometers at Kew Observatory and their Discussion. By C. Chree, Sc.D., LL.D., F.R.S., Superintendent	401
On the Heat dissipated by a Platinum Surface at High Temperatures. By J. E. Petavel, 1851 Exhibition Scholar. Communicated by Lord Rayleigh, F.R.S.	403
On a New Constituent of Atmospheric Air. By William Ramsay, F.R.S., and Morris W. Travers.....	405
On the Position of Helium, Argon, and Krypton in the Scheme of Elements. By Sir William Crookes, F.R.S.....	408

No. 400.

Meeting of June 16, 1898, and List of Papers read.....	412
Observations on Stomata. By Francis Darwin, F.R.S.	413
Mathematical Contributions to the Theory of Evolution. V. On the Reconstruction of the Stature of Prehistoric Races. By Karl Pearson, F.R.S., University College, London	417
The Nature of the Antagonism between Toxins and Antitoxins. By C. J. Martin, M.B., D.Sc., Lond., Acting Professor of Physiology, and Thomas Cherry, M.D., M.S., Melb., Demonstrator and Assistant Lecturer in Pathology in the University of Melbourne. Communicated by D. Halliburton, F.R.S.	420
On the Source of the Röntgen Rays in Focus Tubes. By Alan A. Campbell Swinton. Communicated by Lord Kelvin, F.R.S.....	432
On the Companions of Argon. By William Ramsay, F.R.S., and Morris W. Travers	437
Summary of the Principal Results obtained in a Study of the Development of the Tuatara (<i>Sphenodon punctatum</i>). By Arthur Dendy, D.Sc., Professor of Biology in the Canterbury College, University of New Zealand. Communicated by Professor G. B. Howes, F.R.S.	440
The Stomodæum, Mesenterial Filaments, and Endoderm of <i>Xenia</i> . By J. H. Ashworth, B.Sc., Demonstrator in Zoology, Owens College, Manchester. Communicated by Professor Hickson, F.R.S.	443
On Surfusion in Metals and Alloys. By W. C. Roberts-Austen, C.B., D.C.L., F.R.S. (Plates 8, 9)	447
Experimental Investigations on the Oscillations of Balances. By D. Mendeléeff, <i>For. Mem. R.S.</i>	454

	Page
On the Determination of the Magnetic Susceptibility of Rocks. By A. W. Rücker, Sec.R.S., and W. H. White, A.R.C.S.	460
On the Detection and Localisation of Phosphorus in Animal and Vegetable Tissues. By A. B. Macallum, Associate-Professor of Physiology, University of Toronto. Communicated by Professor Sherrington, F.R.S.	467
Falmouth Magnetic Observatory. Note	480

Obituary Notices :—

Prof. Hubert A. Newton	i
Sir Richard Quain	vi
James Joseph Sylvester (with Portrait).....	ix
Alfred Louis Olivier Le Grand Des Cloizeaux	xxv
John Carrick Moore	xxix
Baron Ferdinand von Mueller.....	xxxii
Index	xxxvii
Erratum	xliii

PROCEEDINGS

OF

THE ROYAL SOCIETY.

March 3, 1898.

SIR JOHN EVANS, K.C.B., D.C.L., Treasurer and Vice-President,
in the Chair.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Allen, Alfred Henry, F.C.S.	Gray, Professor Thomas, B.Sc.
Baker, H. Brereton, M.A.	Haddon, Alfred Cort, M.A.
Baker, Henry Frederick, M.A.	Hamilton, Professor David James, M.D.
Barrett, Professor W. F., F.R.S.E.	Harmer, Sidney Frederic, M.A.
Binnie, Sir Alexander Richardson, M.Inst.C.E.	Head, Henry, M.D.
Bovey, Professor Henry T., M.A.	Hiern, William Philip, M.A.
Bridge, Professor Thomas William, M.A.	Kanthack, Professor Alfredo A., M.D.
Brown, Professor Ernest William.	Lansdell, Rev. Henry, D.D.
Bruce, Surgeon-Major David, M.B.	Lewes, Professor Vivian B., F.C.S.
Buchan, Dr. Alexander, M.A.	Lewis, W. Bevan, M.R.C.S.
Burch, George James, M.A.	Lister, Arthur, F.L.S.
Callaway, Charles, D.Sc.	Lister, Joseph Jackson, M.A.
Cardew, Major Philip, R.E.	MacArthur, John Stewart, F.C.S.
Corfield, William Henry, M.D.	MacGregor, Professor James Gordon, D.Sc.
Crookshank, Professor Edgar March, M.B.	McMahon, Lieutenant - General Charles Alexander.
David, Professor T. W. Edgeworth, B.A.	Mallock, Henry, Reginald Arnulph.
Dixon, Professor Alfred Cardew, M.A.	Mance, Sir Henry C., C.I.E.
Dixon, Professor Augustus Edward, M.D.	Mansergh, James, M.Inst.C.E.
Gamble, James Sykes, M.A.	Marsh, James Ernest, M.A.

Matthey, Edward, F.C.S.	Smithells, Professor Arthur, F.C.S.
Mill, Hugh Robert, D.Sc.	Spencer, Professor W. Baldwin, B.A.
Morgan, Professor Conwy Lloyd, F.G.S.	Starling, Ernest Henry, M.D.
Muir, Thomas, M.A.	Stockman, Professor Ralph, M.D.
Muirhead, Alexander, D.Sc.	Swinton, Alan Archibald C., Assoc. M.Inst.C.E.
Notter, Surgeon-Lieut.-Col. James Lane.	Symington, Professor Johnston, M.D.
Oliver, Major-Gen. John Ryder, R.A.	Tanner, Professor Henry William Lloyd, M.A.
Osler, Professor William, M.D.	Taylor, Henry Martyn.
Parsons, Hon. Charles A., M.A.	Thomas, Michael Rogers Oldfield, F.Z.S.
Perkin, Arthur George.	Threlfall, Professor Richard.
Preston, Professor Thomas, M.A.	Tutton, Alfred E., F.C.S.
Rambaut, Arthur A., M.A.	Walker, Professor James, D.Sc.
Reid, Clement, F.G.S.	Waterhouse, Colonel James.
Reid, Professor Edward Waymouth, M.B.	White, William Hale, M.D.
Salomons, Sir David, Bart., M.A.	Whympier, Edward, F.R.G.S.
Scott, Alexander, M.A.	Wimshurst, James.
Seward, Albert Charles, M.A.	Windle, Bertram Coghill Allen, M.D.
Shenstone, William Ashwell, F.I.C.	Woodhead, German Sims, M.D.
Smith, Professor William Robert, M.D.	

The following Papers were read :—

- I. "The Relationship of Variations of the Ground-water Level to the Incidence of Malarial Fevers in Chotta Nagpur, Bengal." By Dr. L. ROGERS. Communicated by Dr. LAUDER BRUNTON, F.R.S.
- II. "On the Depletion of the Endosperm of *Hordeum vulgare* during Germination." By H. T. BROWN, F.R.S., and F. ESCOMBE.
- III. "On Apogamy and the Development of Sporangia upon Fern Prothalli." By W. H. LANG. Communicated by Professor BOWER, F.R.S.
- IV. "Experimental Observations on the Early Degenerative Changes in the Sensory End-organs of Muscles." By Dr. F. E. BATTEN. Communicated by Professor V. HORSLEY, F.R.S.

“On the Depletion of the Endosperm of *Hordeum vulgare* during Germination.” By HORACE T. BROWN, F.R.S., and F. ESCOMBE, B.Sc., F.L.S. Received December 11, 1897,—Read March 3, 1898.

[PLATE 1.]

In an account given by one of us in 1890 of the results of an investigation of the histological and physiological changes which take place in the seeds of the Grasses during germination,* a prominent position was given to a discussion of the relations existing between the endosperm and embryo, and to the part played by each in the preparation of the reserve materials of the seed for the nutrition of the young plant. This branch of the inquiry was much facilitated by the discovery that the embryo, when separated from the other parts of the seed, is capable of an independent existence, providing it is supplied with a suitable artificial nutriment in the form of certain carbohydrates, its own store of proteids being sufficient to supply the nitrogen requisite for the production of young plants of a considerable size.

The carbohydrates most favourable to rapid growth in such cases are sucrose, dextrose, and maltose; but it was also found that the embryo, when deprived of such readily assimilable material, acquires the power of dissolving solid starch to a very notable extent, a function which was subsequently localised in the columnar epithelium of the scutellum.

The endosperm itself was also subjected to examination with a view to determine if it possesses any power of acting on the reserve materials contained within its cells, and of bringing about any self-depletion which is independent of the influence of the embryo.

This question was attacked in two different ways. In the first place, endosperms, after being degermed, were placed under favourable conditions for the full play of any metabolic activity which might be possessed by any portion of their tissue, every facility being afforded for the rapid outward diffusion of the products of change; and, secondly, advantage was taken of a fact which had previously been established, that an embryo may be transferred from one endosperm to another without materially affecting its power of subsequent growth; thus affording an opportunity of subjecting an endosperm to such treatment as may reasonably be supposed capable of destroying any residual vitality in its cells, and of then observing how this affects the subsequent development of a fresh embryo “grafted” upon it.

For the full details of these experiments we must refer to the

* Brown and Morris, ‘Chem. Soc. Journ.,’ vol. 57, p. 458.

4 Messrs. Brown and Escombe. *On the Depletion of the*

original paper, and here merely quote the general conclusions which were drawn from them.

Although the peripheral layer of the endosperm, the so-called "aleurone-layer," or "Kleberschicht," undoubtedly consists of living cells, no evidence could be obtained of the existence of any residual vitality in the amyliiferous cells, which constitute by far the greater portion of the endosperm.

No changes were observed in the isolated endosperms in the direction of self-depletion which were comparable in intensity with those produced when the embryo was attached; and when those changes did occur they were always preceded by an invasion of bacteria and moulds in the culture-medium, the disintegration and dissolution of the endosperm-contents in such cases proceeding in such a manner as to suggest that they were conditioned entirely by the organisms.

When living embryos were "grafted" on endosperms which had remained in alcohol for six months, and in which it was then reasonable to suppose that any residual vitality had been effectually destroyed, all the usual phenomena incident to normal germination were observable in those endosperms. Hence it was concluded that the idea of any co-operation on the part of the endosperm-cells was superfluous, and that the determining factor in the normal endospermous changes is the embryo itself, which, by independent experiment, had been shown to possess the power of dissolving starch and of initiating those phenomena of cytohydrolysis which are amongst the earliest exhibited in natural germination. According to this view the endosperm of *Hordeum*, and probably of all the Grasses, is, as far as its starch-containing cells are concerned, a *dead* storehouse of reserve material, whose stores can be converted to the use of the plant by the action of the embryo only, and that this, for a limited period of its existence, lives a truly saprophytic life.

These conclusions are in accord with the views of Van Tieghem, that, whilst an endosperm, such as that of *Ricinus*, containing oil and aleurone as reserve materials, is endowed with a vital activity of its own, by virtue of which it is capable of digesting the reserve material in preparation for the embryo, the endosperm of seeds, whose reserve materials, on the other hand, consist of starch and cellulose, remains passive during germination, the digestion of its reserves being in this case effected by the embryo.

Two years after the appearance of the above-mentioned paper, Pfeffer gave a brief description of some work by B. Hansteen, 'Über die Ursachen der Entleerung der Reservestoffe aus Samen,'*

* 'Ber. der Königl. Sächs. Gesellsch. d. Wissenschaften zu Leipzig,' 1893, p. 421.

which was followed the year afterwards by a more detailed paper on the same subject by Hansteen himself.*

Hansteen strenuously opposes Van Tieghem's division of endosperms into "active" and "inactive," and asserts that the latter was led into error on this point by not taking precautions to put his endosperms under conditions favourable for the rapid removal of the products of change. Although reference is made to the Brown and Morris paper of 1890, the author does not appear to have made himself thoroughly acquainted either with the details of the experiments described or with the conclusions drawn from them.

Hansteen's principal experiments were made with the seeds of *Zea Mays* and *Hordeum vulgare*, but he also made observations on the mucilaginous endosperm of *Tetragonolobus purpureus*, and the cotyledons of *Lupinus luteus* and *Helianthus annuus*. For our present purposes it is only necessary to consider the experiments on barley and maize. The seed was, in the first instance, soaked in water for two days, and the embryos, including the scutellum, were removed. To the isolated endosperms there was then applied a mixture of plaster of Paris and water, so as to form a small plaster column, which occupied the original position of the embryo.

The little plaster columns, with the endosperms attached, were then put into glass dishes containing a sufficient amount of water to reach half-way up the columns. In order to avoid the disturbing influence of micro-organisms the seeds were placed for two hours in a 1 per cent. solution of copper sulphate; all the materials and vessels used were carefully sterilised, and the experiments were performed under strict antiseptic conditions in a cultivation chamber so arranged as only to admit germ-free air. The author states that he has been able in this manner to maintain his cultures sterile for at least a month. When there was a sufficient amount of water in the culture-dishes, and the conditions were thus favourable for a rapid outward diffusion of the products of change, Hansteen found that, within from ten to thirteen days of commencing the experiment, the isolated endosperms of both maize and barley had given rise to a very considerable self-digestion of the cell-contents. In the immediate neighbourhood of the plaster the cells had quite lost their starch, whilst the starch-granules, even at a distance, were more or less corroded, and the partially depleted endosperm had become soft and disintegrated. In the case of barley these visible changes were very strongly marked indeed, and simultaneously with them sugar could be detected in the water into which the small plaster columns dipped.

In those experiments in which the amount of water had been much reduced, but very little starch-erosion took place at the point

* 'Flora,' vol. 79, 1894, p. 419.

of contact with the plaster, a fact which the author attributes to the accumulation within the endosperm of an excess of soluble products, which thus exercise an unfavourable influence on the continuous chemical change of the solid reserve substances.

In view of Haberlandt's assertion that the cells of the "Kleberschicht" have a distinct diastase-secreting function, Hansteen experimented in a similar manner with endosperms which had been deprived of this layer, and he found the same indications of self-depletion as before. He therefore concluded that the dissolution and depletion which he had observed are due to a special activity of the inner starch-bearing cells of the endosperm. The question is then discussed whether, during germination, the embryo does or does not secrete an enzyme, and the conclusions arrived at are in accord with those of Brown and Morris, and Grüss, that such a secretion does take place. Hansteen, again agreeing with the former observers, regards this secretion of diastase as conditioned by the falling off in the supply of readily soluble carbohydrates; whether, however, the diastase so produced plays an important part in normal depletion, or whether the asserted self-depletive power of the endosperm-cells is sufficient, in normal germination, to account for all the observed results, the author leaves an open question.

In a long memoir entitled "Beiträge zur Physiologie der Keimung,"* J. Grüss discusses the question of the appearance of ferments in the endosperms of maize and barley after excising the embryos and filing off the "Kleberschicht" ("aleurone-layer"). From experiments made by burying fragments of such endosperms for a few days in sterilised moist sand, he concludes that the starch-bearing cells have the power of producing spontaneously within themselves a diastase, the presence of which he determined, in the first place microscopically by the extremely doubtful guaiacum-reaction, and secondly by the increased action of the endosperm-tissue on thin starch-paste.

The most recent contribution to the subject is a paper, taking the form of a preliminary communication, by K. Puriewitsch, "Ueber die selbstthätige Entleerung der Reservestoffbehälter,"† followed since by a more detailed paper, entitled "Physiologische Untersuchungen über die Entleerung der Reservestoffbehälter."‡ Making use in the main of Hansteen's method of experiment, Puriewitsch examined, amongst other seeds, the isolated endosperms of *Zea Mays*, *Triticum sativum*, *Hordeum distichon*, *Secale cereale*, and *Oriza sativa*, and he extended his observations to the cotyledons, bulbs, rhizomes, and roots of various other plants, a list of which is given in his paper.

* 'Landwirtschaft. Jahrbücher,' 1896, p. 385.

† 'Ber. Deut. Bot. Gesell.,' vol. 14, 1896, p. 207.

‡ 'Pringsheim's Jahrb.,' vol. 31, 1897, p. 1.

In the case of *maize* he found the first indications of action in the cells lying next the scutellum, and this gradually extended along the periphery of the endosperm until, within fourteen or fifteen days, this was completely emptied of its contents, with the exception of a few cells in the central portions. He states that this action is not due to any direct influence of the plaster, as suggested by Grüss, since it takes place also in contact with water only. In the cotyledons of *Lupinus* the depletion takes place even with greater rapidity than in normal germination, and no difference is observed whether the cut surface in contact with the water or gypsum, as the case may be, is on the side adjacent or opposite to the axial organs. With the isolated endosperms of maize and wheat, on the other hand, Puriewitsch states that the case is different, since self-depletion proceeds much more rapidly through the surface originally in contact with the scutellum than it does from the opposite side. The author also found that the depletion of the endosperm is much retarded in the case of maize and wheat by the presence in the water of 2 per cent. of dextrose or glycerine, or by 3 per cent. of cane sugar, and that it is completely arrested by 1.5 per cent. of sodium chloride or potassium nitrate. The results on the whole are regarded as contradicting the conclusions of Brown and Morris that the endosperm is merely an inactive storehouse of reserve material, and Puriewitsch considers that this is further borne out by the behaviour of isolated endosperms in an atmosphere of water and by the action of anæsthetics such as ether and chloroform. Under these latter conditions, he states that the endosperms of maize and wheat remain unchanged, but that the depletive action recommences as soon as the disturbing influences are removed. Attempts were made, by applying food material in the form of weak sugar solutions, to induce a re-deposition of reserve material in the self-depleted tissue. These attempts were wholly unsuccessful in the case of maize and wheat, but the emptied cotyledons of *Lupinus albus* and *Phaseolus multiflorus*, the bulbs of *Hyacinthus orientalis*, and the rhizomes of *Curcuma amadu* and *Iris germanica* were all capable of re-forming starch within their cells.

It will be noticed that in the recent work of Hansteen, Pfeffer, Grüss, and Puriewitsch, there is a general agreement that the amyli-ferous cells of the endosperm of the Grasses have a definite power of digesting their reserve materials, this power being entirely independent of any influence of the embryo, and the only necessary condition for its exhibition being that the products of metabolism shall not be allowed to accumulate within the endosperm. The conclusion is, in fact, that the starch-bearing endosperm-cells are still living units, just as are the cells of the cotyledons of *Lupinus*, *Phaseolus*, and *Bicinus*, which are admitted on all hands to have self-depletive power.

As these conclusions are in many respects opposed to those arrived at by one of us a few years ago, we have considered it necessary to institute a further series of experiments, and to re-examine the whole question of the mutual dependence of the embryo and endosperm. In doing this we have endeavoured to free our minds of any bias which might, even unconsciously, have been given by our previous experiments, and to subject those experiments to the strictest possible criticism.

Broadly speaking, the question resolves itself into a consideration of the various causes at work in bringing about the solution of the reserve material of the seed in preparation for its absorption by the scutellum of the young plant, and the due apportionment of this work to (1) the embryo itself, (2) the amyliiferous cells, and to (3) the peripheral cells of the endosperm, the so-called "aleurone-layer" or "Kleberschicht."

In addition to this, we have to take into account the possibility of some of the changes being brought about by enzymes pre-existent in the amyliiferous cells, which may be altogether independent of the present life of the cytoplasm. We have, further, to determine the part played by micro-organisms accidentally brought into action during the experiments, and to eliminate the changes due to their influence alone.

In work of this character we can only attain to results of any value by a great multiplication of experiments made in such a manner as to admit of the close and frequent comparison of different series performed under every conceivable variation of conditions.

All our new work was conducted on barley only, and the results are based on very many hundreds of experiments, extending over a period of more than twelve months, during which time various possible sources of error were gradually excluded.

As long as we confine our attention to intact seeds the disturbing influence of micro-organisms is but small, but the case is different when the seed envelopes have to be cut through and the embryo removed, the endosperm, thus bared and deprived of its protective coatings, being then open to the attack of bacteria and moulds, which thrive in the culture-medium employed, and by their action induce changes in the contents and cell-membranes of the endosperm-cells which it is almost impossible to distinguish from those initiated by the cells themselves, supposing them to be living and active units.

At the outset of the investigation we spent a considerable time in endeavouring to find some antiseptic agent possessed of such a differential action as to inhibit, or at any rate to materially retard, the growth of micro-organisms, whilst not interfering with the normal growth of vegetable organs. Many various reagents were tried,

commencing with extremely dilute solutions, which were gradually increased in strength until their influence on the germinative power of the seed was just perceptible. The germicidal effect of such a solution was then tested on degermed grains in water-culture. At one time extremely dilute solutions of formaldehyde and of acid potassium fluoride offered some hope of success in this direction, but neither of these substances on further investigation gave a sufficient differential action to be of any practical use.

In the experiments of 1890 (Brown and Morris, *loc. cit.*) the disturbing effect of micro-organisms was minimised by restricting the time of the experiment as far as possible, and by sterilising the culture-media, and we have seen that Hansteen relied on killing the adherent germs with a solution of copper sulphate, and on the employment of strict antiseptic methods, even to the extent of carrying out all the operations in a germ-free atmosphere.

We have made experiments in order to see how far such a treatment with copper sulphate effects sterilisation of the integuments, the grain after such treatment being incubated in contact with vegetable infusions. The results have clearly shown us that although such a procedure may retard the subsequent development of *Bacteriaceæ* and moulds, it is impossible by means of it to ensure a complete destruction of all the germs adherent to the paleæ, unless the treatment is sufficiently prolonged to destroy, or at any rate to materially reduce, the germinative power of the embryo.

Since any process which will affect the vitality of the embryo cannot be without some similar influence on the endosperm, there is thus introduced an element of uncertainty into all subsequent processes which may be devised for determining whether the amyliferous cells are living or dead.

Extreme refinements for avoiding air-sown organisms are obviously of little efficacy when complete initial sterilisation of the exterior of the grain cannot be ensured. Nevertheless, many of our experiments have been carried on with precautions of this kind, but have not yielded better results than those made in covered dishes with sterilisation of the culture-media and apparatus.

In all experiments with endospermous seeds deprived of their embryos both Hansteen and Pfeffer have, very properly, laid great stress on the necessity for providing for a rapid removal of any possible products of change in the isolated endosperm as fast as they are formed, but these observers have apparently entirely overlooked the fact that this was fully insisted upon and provided for in the earlier experiments described by one of us in 1890.* The plan adopted was to insert the proximal ends of the degermed grains into small holes made in a thin mica plate, which was then floated

* '*Chem. Soc. Journ.*,' 1890, Trans., p. 281.

on water in such a manner as to just immerse that portion of the endosperm which had been in contact with the embryo.

This method really affords much greater facilities for outward diffusion from the endosperm than does Hansteen's plan of fixing the degermed seeds on small columns of plaster partially immersed in water, and it is also free from the objection of any possible disturbing influence due to the solubility of the plaster. Moreover, the mica-raft method is easier of manipulation, and whilst giving perhaps better facilities for sterilisation, also allows the detection of the very first appearance of micro-organisms.

The barley used in our experiments was *Hordeum vulgare* (var. *distichon*), derived from two sources. One, with which most of the work was done, was a well-matured Chilian barley, of the Chevalier type, the other an English Chevalier barley grown on light land in Northamptonshire, both samples being well matured and well harvested.

It will be convenient in the first place to consider the visible changes which can be induced in the endosperm when this is completely deprived of its embryo, and is put under such conditions as to ensure the speedy removal of any soluble and diffusible products which may result from any self-digestive processes initiated by any portion of the endosperm tissue.

Some of our experiments on this point were made in the following manner:—

The grain was, in the first place, steeped from one to two hours in a 1 per cent. solution of copper sulphate, and after being washed with sterilised water was steeped, also in sterilised water, for a period of from twenty-four to forty-eight hours. From the corns selected for experiment the paleæ and embryos were then removed with antiseptic precautions, this process being conducted in a glass-fronted sterile operating chamber, furnished with "sleeves." The degermination was performed with a small scalpel, taking care to thoroughly remove all traces of the scutellum, and to lay bare the "depleted layer" of the endosperm.*

The isolated endosperms were then put in position in small holes made in a very thin mica-raft which was floated on sterilised water in a Petri's dish, or in a glass vessel of somewhat similar construction.

* The nature and origin of this "depleted layer" can only be understood by following the developmental history of the endosperm and embryo, and this has been so fully described in the Brown and Morris paper of 1890 (*loc. cit.*) that it requires but a passing notice here. The "depleted layer" is made up of several thicknesses of cell-membrane, which originally formed part of the amyliiferous cells of the young immature endosperm. During the later stages of development of the grain, and some time before maturation, the contents of these cells are used up for the nutrition of the young embryo, but the cell-membranes persist and become squeezed together by the gradual encroachment of the scutellum.

In those cases where comparisons had to be made between endosperms treated in different ways the mica-rafts were made to carry twelve corns, the two series of six each being placed on either side of the raft. In this manner there was an exactly equal chance of the two sets being infected to the same extent by extraneous organisms, an important condition, which often enabled us in a long series of experiments to differentiate changes due to the influence of organisms from those due to other causes. Latterly we found these extreme antiseptic precautions unnecessary for the reasons already given, and we also found it undesirable to previously steep the grain before degermination, since the embryo may readily begin to function slightly during the softening process, especially when the temperature is high. In such cases there is a danger of the projection of a small quantity of enzymes from the embryo into the proximal portions of the endosperm, and these enzymes, after degermination of the grain and the floating of the endosperms on the rafts, may give rise to certain changes in the endosperm which may be wrongly attributed to a self-digestive power of the endosperm-cells themselves, whereas they have a different origin altogether.

It is true that this source of error may be minimised by reducing the period of steeping, and by keeping the temperature of the water low; but it is much more satisfactory to degerm the grain *whilst still in its dry resting condition*, a process which does not present any difficulty. It must, however, be performed with the aid of a lens, so as to ensure the complete removal of the scutellum and the whole of its limiting epithelial layer.

If endosperms thus treated are soaked in water for from twenty-four to forty-eight hours, and are then transferred to the perforated mica-rafts in such a manner as to immerse the whole of the depleted layer, we observe the following changes to take place.

Within two or three days from the commencement of the experiment the peripheral, tripartite layer of the endosperm, the so-called "aleurone-layer" ("Kleberschicht"), shows an increasing tendency to separate from the adjacent amyliiferous cells. This is noticeable in the first instance at the proximal end of the endosperm, on the dorsal side,* where the "aleurone-layer" is intersected by the "depleted layer," and whilst it is to some extent traceable for some distance round the periphery towards the ventral fold, it extends much more rapidly in a distal direction along the dorsal side. Where there is this megascopic indication of the separation of the "aleurone-layer," it is always found that the amyliiferous cells, immediately underlying,† show indications of change. In the first

* The dorsal side is that immediately opposite the ventral suture. The terms *proximal* and *distal* are used with reference to the position of the embryo.

† The outermost layer of the amyliiferous portion of the endosperm consists of

place, the cell-contents become hyaline in appearance, owing to the protoplasmic matrix losing its granularity and acquiring a refractive power approximating to that of the embedded starch-granules. Later on these hyaline portions imbibe water and swell up enormously, ultimately becoming very elastic and ductile, and capable of extension into sticky, stringy masses, very similar in appearance to the gluten of the wheat-endosperm. We shall in future refer to this change as "gluten-formation." At the same time the cell-membrane of the peripheral starch-cells swells up considerably, and as the action progresses the cell-walls undergo disintegration, with all the indications of cytohydrolysis as described by Brown and Morris.

It is to this cytohydrolysis that the separation of the "aleurone-layer" is due, and the disintegration due to this cause proceeds centripetally into the endosperm and extends round the periphery nearly to the ventral fold, whilst it advances more rapidly in a distal direction on the dorsal side. The extent to which this cytohydrolysis has proceeded is always evidenced megascopically by the reduction of the endosperm-contents to a "mealy" consistency, but even after the lapse of seven or eight days the actual amount of depletion is small, as long as micro-organisms are absent, or present only in comparatively small numbers. If, however, as is frequently the case, masses of *Bacteriaceæ* in the zoogloea-state attach themselves to the mutilated surface of the endosperm, a very distinct removal of some of the endosperm-contents may take place.

The erosion of the starch-granules is generally not very pronounced under these conditions, but when it does occur it always commences at the same point as the cytohydrolysis, that is, on the dorsal side, at the angle of intersection of the "aleurone-layer" and the "depleted layer," and extends distally just as does the cytohydrolytic action.

The starch-erosion produced in this manner under the "aleurone-layer" is, in the main, very different in character from that observed immediately under the scutellum of a grain germinating normally with its embryo attached. Whilst in the latter case the action commences by the formation of numerous minute "pits," this preliminary pitting is rarely observable in the eroded granules lying under the "aleurone-layer," which show the production of large rifts, and a general concentric dissolution of the various layers. We shall in future refer to these different modes of attack on the starch-

cells differing in general appearance from the more deeply seated cells. They are smaller, are packed with far smaller starch-granules, and the proportion of starch-granules to proteinic contents is less. These peripheral cells constitute the youngest part of the starchy endosperm, and may be regarded as having been arrested in their development by the falling off in the supply of formative material at the *period of maturation*.

granule as "sub-aleuronic" and "sub-scutellar" respectively; for although occasional instances may occur where one form of attack merges insensibly into the other, yet, looked at generally, they differ so much from each other as to suggest that the transforming agents are essentially different.

The accompanying photographs (Plate 1) illustrate these differences far better than can any mere description.

It appears to us that the phenomena which are observed when the endosperms of *Hordeum* are deprived of their embryos, and are treated in the manner we have described, must be attributable to one or more of the following causes:—

1. They may be the result of micro-organisms originating in the culture-medium, and gradually invading the endosperm-tissues, which undergo progressive alteration either by the direct action of the organisms or in virtue of their secreted enzymes projected into the endosperm.

2. The phenomena may be due to residual enzymes, cytohydrolytic, amylolytic, and proteolytic, left in the endosperm at the time of maturation and desiccation of the grain.

3. They may be due to the revival of metabolic activity of still living cells of the endosperm when these are placed under favourable conditions of moisture and temperature, and facilities are afforded for the removal of the products of change. If this is the correct solution the active cells may be those of (a) the "aleurone-layer," or (b) the amyliferous portions of the endosperm.

We must now consider these three possibilities in detail.

We have already stated that, no matter how careful we may be in sterilising the apparatus and culture-medium, the appearance of micro-organisms is only a question of time, unless we employ anti-septic methods of so drastic a nature as to seriously imperil the vitality of the endosperm-tissue, a course which would render it impossible to get the answer we require as to the respective parts played by organisms and by autonomous changes in the endosperm-cells themselves. We can, however, arrive at certain conclusions by making a large number of experiments and by confining our observations to the period prior to the appearance of organisms, a period which, under favourable circumstances, may extend to about eight days. When this is done we find that the changes originating in the first place under the "aleurone-layer" of the degermed seeds so far precede in point of time the appearance and multiplication of the *Bacteriaceæ* and moulds as to render it in the highest degree improbable that the two sets of phenomena are causally related to each other.

A much more satisfactory proof of the truth of this proposition may be obtained in an entirely different manner. Endosperms of

14 Messrs. Brown and Escombe. *On the Depletion of the*

barley which have been degermed in the dry state are, in the first place, steeped in a saturated aqueous solution of chloroform for twenty-four hours. After having freed the endosperms from adherent moisture, they are warmed gently for a few hours in a flask connected with a water-pump, and are then steeped for a further period of twenty-four hours in running water, every trace of chloroform being thus removed. The endosperms are then floated in the usual manner on a mica-raft, alongside other degermed endosperms which have been merely steeped in water for forty-eight hours. The two sets of endosperms are thus under exactly similar conditions as regards their liability to attack by micro-organisms, and if the described "sub-aleuronic" changes of the endosperm are due solely to the direct or indirect influence of extraneous organisms the same results ought to be given by the two series, whereas, if the vitality of any portions of the endosperm is a determining factor, evidence of this ought to be forthcoming, since one set of endosperms has been under conditions which would completely arrest the vital functions of any of their component cells.*

When such an experiment is performed we find very considerable differences between the two sets of endosperms at all stages. Whilst the series merely steeped in water go through the ordinary cycle previously described in detail, the series made up of the chloroformed endosperms show no internal changes for a considerable period of time. In the latter case the "aleurone-layer," which so speedily separates under ordinary conditions, retains its unbroken continuity with the subjacent amyloiferous cells, which in their turn preserve their cell-walls and cell-contents intact. Until the growth of micro-organisms has progressed to a very considerable extent the endosperm-contents of the chloroformed grains show no megascopic or microscopic change, except in the direction of a more hyaline appearance of the contents of the starch-cells, a change which is apparently the first stage of the "gluten-formation," to which reference was made in an earlier part of the paper. There is neither cytohydrolysis nor amylohydrolysis apparent in the tissues until the micro-organisms which have attached themselves in a zoogloea state to the outside of the "depleted layer" have attained to a very luxuriant growth, and even then the tissue-changes differ in some important particulars from those produced in a "living" endosperm. It is in fact possible,

* In our earlier experiments it was assumed that a treatment with chloroform-water, sufficient to destroy the vitality of the embryo, would also be sufficient to kill the aleurone-cells. This, however, is not the case, the embryo being much more sensitive to the chloroform than the peripheral cells of the endosperm. We have satisfied ourselves, however, that a twenty-four hours' steeping of the *dry* endosperms in chloroform-water at a temperature not less than 15° C. will *permanently destroy the functioning power of all the cells of the grain.*

by such comparative experiments, to differentiate with certainty the modifying action of micro-organisms from the autonomous action of the endosperm-cells themselves.

The action due to extraneous organisms always commences at the surface of the "depleted layer," the cell-membranes of which this is made up being softened, swollen, and ultimately disintegrated. This cytohydrolytic action then gradually extends to the membranes of the amyloiferous cells, and the proteid contents of the cells are also involved in the change, which ultimately permeates the whole of the endosperm.

There is, however, a striking difference between the mode of progression of this bacterial action from that observed in "living" degermed endosperms. In this latter case, as we have already noted, the action is essentially centripetal, commencing under the "aleurone-layer" on the dorsal side, where this layer is intersected by the "depleted layer," and extending peripherally and axially, but more rapidly on the dorsal side. In the degermed "dead" endosperms, on the other hand, there is no differential progression of this kind, since the action, whilst progressing in an axial direction, does not extend more rapidly along the peripheral than the central parts, and does not show the slightest tendency to more rapid extension on the dorsal side, a tendency which is so strongly marked in "living" degermed endosperms in water-culture, or in intact grains of barley undergoing ordinary germination. It is only when the disintegration of the endosperm-contents under the action of micro-organisms has proceeded to a very considerable extent that any notable amount of erosion of the starch-granules is observable. This sometimes does not occur for many days, a fact probably due to the bacteria not secreting any special starch-dissolving enzyme as long as they are well supplied with readily assimilable food material from other sources.

So far the conclusions are altogether opposed to the view that the normal phenomena of endosperm solution and depletion, as they occur in degermed endosperms in water-culture, can be explained by the action of extraneous micro-organisms. It is true that, under certain circumstances, the mixed growths of *Bacteriaceæ* which attach themselves to the mutilated surface of the endosperm can induce changes in the subjacent tissues by the projection into them of certain enzymes, the products of their growth, but this action can, with due care, be clearly differentiated from the normal action, which is of quite a different character, and must be in some way self-induced by the endosperm-cells themselves.

Before considering how far the normal changes are dependent on the vitality of any particular portion of the endosperm, we must inquire if the phenomena are in any way due to enzymes pre-existent

in the endosperm-cells, and this inquiry is the more necessary since we know that even the distal portions of the endosperms of the barley-grain contain a certain amount of a feeble diastase, and in most cases also a distinct amount of a cytohydrolytic enzyme.*

In the first place we satisfied ourselves that both the amylohydrolytic and cytohydrolytic enzymes of barley are not appreciably weakened in their respective actions by a saturated aqueous solution of chloroform.† A number of grains of barley were degermed, and, after being softened by a sufficiently long steep in chloroform-water, were placed in the usual manner on a mica-raft, which was floated on water kept fully saturated with chloroform during the whole of the experiment. Under these conditions bacterial growth was quite inhibited, as was also any autonomous action due to the endosperm-cells, but the pre-existent enzymes, on the other hand, were allowed full play to produce any alterations of which they were capable.

Not even the feeblest action of any kind could ever be detected in the endosperm-tissue placed under these conditions, even after the lapse of several weeks, and we must therefore regard such experiments as fatal to the view that *pre-existent enzymes* exercise any appreciable influence in bringing about the well-marked and definite changes in the endosperm such as we have described.

We are thus led to what appears to be the only conceivable explanation remaining,—that the phenomena are dependent on the metabolic activity of some portion of the endosperm itself; and if this is the case, it follows that during normal germination the endosperm is not wholly passive, but takes some share with the embryo in preparing the reserve materials for the use of the young plant.

It now remains to ascertain how far it is possible to localise the particular part of the endosperm-tissue which is active in producing these changes, an inquiry which resolves itself into an examination of the respective functions of the “aleurone-layer” and amyliiferous cells respectively.

The observations of Tangl,‡ and more recently those of Haberlandt, have shown that each of the “aleurone-cells” possesses protoplasm with the usual reticulation of fine strands, enclosing a well-defined nucleus, and presenting all the usual cytological evidences of activity. As far as we are aware, no one who has ever carefully

* ‘Chem. Soc. Journ.,’ 1890, p. 507; *ibid.*, 1892, p. 362.

† These facts were determined by estimating the diastatic and cytohydrolytic powers respectively of extracts of the grain made under similar conditions, in the one case with water only, and in the other with a saturated aqueous solution of chloroform. The determinations of diastatic activity were made by Lintner’s method, and those of the cytohydrolytic by the times necessary to produce visible action on the cell-membranes of thin sections of the grain immersed in the two liquids.

‡ ‘Sitzungsber. d. Wiener Akad.,’ vol. 102, 1885.

examined these cells during the germinative period has ever doubted that they are actually *living* units.*

The cytological evidence as to the state of the amyliiferous cells is not so clear, and we have been unable to find any record of a systematic examination of the appearances presented by their protoplasmic contents.

The difficulties of examination are, of course, much greater here than they are in the case of the "aleurone-cells," owing to the tightly packed starch-grains, which must be removed by some method incapable of acting on the other cell-contents, which they completely obscure. The ordinary reagents which are used for this purpose, such as acids and alkalis, are quite inadmissible, and although much better results are obtained with cold water extracts of malt, or of animal pancreas, acting for some time at 40—50° C., there are objections to both of these agents. The malt extract often possesses some cytohydrolytic power, which acts on the more delicate portions of the cell-membrane, and destroys the coherence of the tissue, and even when this objection is removed by previously heating the malt extract to 60—65° C. for some time, malt-proteids are often precipitated in a finely granular form within the sections, and confuse the results.

An extract of animal pancreas is a very good solvent for starch, but since this possesses slight proteohydrolytic power in feebly acid solutions, there is a danger of solution of the protoplasmic matrix along with the starch; and, moreover, when "liquor pancreaticus" (Benger) is used, there is considerable precipitation at 40—46° C.

No such objections, however, apply to the use of diluted and filtered mixed† human saliva. With the addition of a little thymol, to prevent putrefaction, this agent may be allowed to act on the very thinnest sections of seeds at a temperature of 46° C. (the optimum temperature for ptyalin) for many hours, without any change in the sections other than the dissolution of the starch. The starch-granules dissolve very completely, leaving sharply marked lacunæ in the protoplasm, which can then be stained in any desired manner.

In staining, we have used a mixture of iodine-green and fuchsine. With this reagent the nucleus is stained green, and is strongly contrasted with the cytoplasm which takes up the red stain.

* Tangl also observed the continuity of the protoplasm in the "aleurone-layer," a continuity effected by means of fine threads passing through pores in the thick walls of contiguous cells. Walter Gardiner ('Roy. Soc. Proc.,' vol. 52, 1897, p. 100) has confirmed this, and informs us that he has also proved the existence of continuity in the cytoplasm of the amyliiferous cells.

† By this is meant ordinary human saliva, consisting of the mixed secretions of the three sets of salivary glands.

When sections of the starch-bearing portions of the mature endosperm are thus treated, it is seen that the nucleus is either extremely deformed, or, indeed, in many cases even completely disintegrated. That these appearances are not in any way due to the treatment to which the sections have been subjected is clearly shown by an examination of sections made from the endosperms of barley, taken from the fields at different stages of development, when starch is still being actively deposited within the cells. In the early stages of development the saliva-treatment gives sections in which normal and well-defined nuclei exist, but as the grain approaches maturity there is a corresponding senescence of the nucleus, resulting in the appearances just described.

It is interesting to trace the progress of this nuclear senescence, which first commences in the more deeply seated and older cells of the endosperm, gradually extending towards the periphery as the period of maturation approaches.

Just before complete ripening, the only well-formed nuclei which can be recognised are those of the last row of starch-bearing cells immediately under the "aleurone-layer." Ultimately, unless some unfavourable circumstances arise to prevent complete maturation, these nuclei to a great extent share the fate of those of the more deeply seated cells, but they are generally deformed to a less degree.

We shall at a future time have more to say on this question as regards other seeds and its connection with the particular nature of the reserve products. The point to which we now particularly wish to draw attention is that the cytological observations indicate the existence of a very marked difference between the nuclei of the "aleurone-cells" and those of the amyliiferous cells. There can be no doubt about the functioning power of the former, whereas it seems difficult to admit that the starch-bearing cells can exercise their full powers as living units after complete maturation, although the destruction of their nuclei may not preclude *all* possibilities in this direction.

Nothing short of actual trial, however, can determine whether the starch-containing cells of the endosperm retain sufficient vitality to have any action on their own cell-membrane or cell-contents, and, with this object in view, we have conducted a number of experiments on large fragments of endosperm deprived completely of their adherent "aleurone-layer" after being steeped for twenty-four hours, and placed under the usual favourable conditions for the rapid outward diffusion of any products of change. For purposes of comparison we also employed other similar fragments which had been *treated with chloroform-water* for a sufficiently long period to effectually destroy any residual vitality, the chloroform being removed

in the same manner as described previously when treating of the intact endosperm.

The results were in no sense doubtful. No visible changes of any kind took place until micro-organisms had established themselves, when dissolution of the cell-membrane commenced. Moreover, there was the strictest possible parallelism, at all stages, between the "dead" and the "living" endospermous fragments, using these terms to express the state, at the commencement of the experiments, of those fragments which had or had not been previously put under conditions for extinguishing any residual vitality which their cells possessed. In this respect our later experiments have fully borne out the statement of one of us in 1890* that the *starch-containing portions of the endosperm* are unable to originate any visible changes in the reserves which they contain.

Thus we must conclude that it is to the influence of the "aleurone-layer," and the "aleurone-layer" only, that we must look for those well-marked changes which undoubtedly take place in the endosperm when this is separated from its embryo and placed under favourable conditions.

This is a conclusion differing materially from that of the 1890 paper referred to above, which concludes with the following passage: "As far as the evidence goes at present, we are certainly not justified even in suspecting that the cells of the 'aleurone-layer' are glandular in the same sense as are the epithelial cells of the scutellum, and until evidence of a far more convincing nature is forthcoming we must adhere to the opinion that the diastase" (and, we might have added, the cytase also) "accumulated in the germinating seeds of the Grasses owes its origin exclusively to the secretory glandular cells forming this scutellar epithelium, and that the aleurone-cells belong solely to the reserve-system of the seed."

This opinion was justified by the known facts of seven years ago, but certainly requires modification in the light of our more recent experiments. It seems, in fact, quite impossible to understand the results of these later experiments, if we deny the power of the "aleurone-layer" to produce a considerable amount of cytohydrolytic action on the cell-membrane, and even a certain amount of action on the starch itself. The relative share in the modification of the endosperm-reserves which falls to the scutellum and the "aleurone-layer" respectively in normal germination we shall consider presently, but it is in the first place necessary to criticise an important experiment of the 1890 paper, which at the time seemed absolutely conclusive against the view that the "aleurone-layer" has any power of modifying the endosperm-contents. Whilst investigating the best conditions for the development of excised

* Brown and Morris, *loc. cit.*

embryos on artificial nutrients, it was found, as we have already stated, that it is possible to "graft" the embryo from one grain on to the endosperm of another, and to obtain such close apposition of the two surfaces, by means of binding with a loop of thin silver or platinum wire, that the "graft" develops into a young plant almost as readily as if it were still nursed by its own endosperm. This fact afforded an opportunity of more closely studying the relative parts played by the embryo and endosperm in producing the initial changes in the reserve materials; for it is evident that if a degermed endosperm is subjected to some process which will with certainty kill its tissue, and a living embryo "grafted" on this endosperm will bring about in the reserve substances of the latter all the changes incidental to normal germination, then the whole idea of residual vitality in the endosperm-cells being a necessary condition of germination would become superfluous. Experiments in this direction were in the first place made by treating grains of barley with chloroform-vapour for twenty-four hours, a course of treatment which we now know must have been insufficient to have killed the resting protoplasts; it is, therefore, not to be wondered at that embryos "grafted" on endosperms so treated should have grown perfectly. In a further set of experiments, also described in the 1890 paper, grains of barley were soaked in absolute alcohol for six months, and after drying off the alcohol, soaking well in water, and degerming, fresh embryos applied to the endosperms were found to produce in them all the ordinary visible signs which accompany germination. This experiment was deemed to conclusively prove that the degradation of the reserve products is conditioned by the embryo itself, and that the endosperm-cells do not take part in it.

We have now, however, every reason to believe that the "aleurone-layer" was *not* killed by this drastic treatment with alcohol, for we have found that these cells are much more resistant to injurious influences than the tissue of the embryo itself, and we have seen cases in which even the embryo will sprout after the grain has been immersed in alcohol for about four months.*

We have recently found, in repeating and varying these experiments, that when the grain is immersed in a dry state in chloroform-water (*i.e.*, a saturated aqueous solution) a few hours suffice to

* Giglioli ('Nature,' vol. 52, 1895, p. 544) found that seeds of *Medicago sativa* retained their vitality after submersion in absolute alcohol for more than *sixteen years*. Ewart ('Liverpool Biolog. Soc. Trans.,' vol. 8, 1894, p. 207) also states that the resistance of seeds to absolute alcohol is very considerable, and that those of *Hordeum*, although killed very quickly by alcohol of 50 per cent., require submersion in *absolute alcohol* for seven weeks before all germinating power is lost. It will be seen, from what has been said above in the text, that the embryos of some grains of *Hordeum* may be made to grow after a much longer submersion than this.

destroy the vitality of the germ, but that at least twenty-four hours' immersion is required to permanently destroy the vitality of the "aleurone-layer," and that if this is not perfectly effected, subsequent "grafting" experiments may suggest entirely erroneous conclusions.

In the following remarks we shall refer to those endosperms which have been thus treated with chloroform-water as "dead," whilst those which have been merely soaked in water after degermination we shall regard as "living."

When "graftings" of embryos are made on living and dead endosperms respectively, and these are placed under favourable conditions for germination, very strongly marked differences are observable within a few days, both in the rapidity of growth and general appearance of the two sets of embryos, and in the nature and extent of the concurrent changes in the endosperms.

On the "living" endosperm the axial organs of the young plant develop freely, healthy rootlets are protruded, and the freely growing plumula has all the appearances of turgidity and firmness incidental to good nutrition. Simultaneously with this development cytohydrolysis commences under the "aleurone-layer," and, whilst attacking the "depleted layer," progresses peripherally and distally along the usual path in the endosperm. At the same time a distinct and sometimes considerable amount of starch-erosion is noticeable in the amyliiferous cells immediately in contact with the "aleurone-layer" of the proximal end of the grain, but this is entirely of a "sub-aleuronic" type (see *antea*), whilst the starch-erosion which has taken place immediately under the scutellum of the "grafted" embryo is wholly of the "pitted" or "sub-scutellar" type.

The phenomena presented by the "grafting" on the "dead" endosperms are, on the other hand, of a very different character. Here the embryo is evidently under much less favourable conditions for healthy growth, since the young plant is much smaller, the tissues of its axial organs are flaccid, and there is very poor root-development. At the same time it is also clear that the embryo is deriving some nutriment from the dead endosperm and is increasing in weight, a fact which can readily be proved by a comparison with the development of excised embryos in water-culture on a porous tile.

The internal changes which the dead endosperm itself undergoes when in contact with the living embryo are very instructive, and a careful study of them enables us, with certainty, to distinguish and delimit the autonomous changes of the endosperm from those induced by the embryo itself. Even after eight or ten days the dead endosperms under these conditions exhibit no softening or cytohydrolysis of the tissues immediately underlying the "aleurone-layer," and this layer remains firmly attached to the subjacent amyliiferous cells.

We can in this case only detect a small amount of disintegration immediately under the "grafted" embryo, and even after eight or ten days this is not found to proceed for more than 0.5 to 1 mm. from the "depleted layer." There is a partial but very incomplete cytohydrolysis of the cell-membranes constituting the "depleted layer," and a similar imperfect action can be traced microscopically in the amyliiferous cells as far as the disintegration has proceeded. The starch-grains of the amyliiferous cells immediately underlying the "depleted layer" show unmistakable signs of attack by normal subscutellar "pitting" without any admixture of that particular form of erosion which is characteristic of the action of the "aleurone-layer."

There can be no doubt that we here have further proof that the embryo, by means of the secretion of enzymes from its scutellar epithelium, is able to attack starch, and to assimilate the products of its hydrolysis. Abundant proof of this fact was brought forward in the 1890 paper, in which were described many experiments on the artificial nutrition of excised embryos, and this fact has been amply confirmed by Grüss in a series of very careful experiments he has recently described.*

It will be remembered that when embryos were cultivated on gelatine in which starch-granules had been suspended, it was found that a secretion of diastatic enzyme took place from the epithelial cells of the scutellum, which manifested itself by erosion of the starch, and that this erosion gradually extended to a relatively considerable depth in the gelatine medium. That this action does not proceed with the same rapidity in "dead" endosperms, on which embryos have been grafted, is due to a great extent to the fact that in this latter case the starch-grains are locked up in cell-membranes which retard the diffusion of the highly colloidal diastase. Until these cell-membranes are broken down we have not the most favourable conditions for a rapid formation of soluble nutriment from the reserve materials, especially as the amyliiferous cells appear to be devoid of any power of initiating such changes autonomously. One of us was originally of the opinion† that the necessary cytohydrolytic function resided in the embryo itself, and that it was manifested by the same epithelial cells as those which produce a very active form of diastase, but our more recent experiments have clearly shown that this power of the embryo was much overrated, and that the greater part of the cytohydrolytic process preliminary to the amylohydrolytic is due to the cells of the "aleurone-layer," the treatment to which the grain was subjected in 1890 not having been sufficient to completely destroy the vitality of these cells.

This layer is the only part of the endosperm which can be recog-

* 'Pringsheim's Jahrb.,' vol. 30, 1897, p. 645.

† *Loc. cit.*, 1890.

nised as taking part in the preparation of the food-material for the embryo, since no evidence can be obtained of any changes being initiated by the starch-containing cells themselves; in fact, the highly disintegrated appearance of the nuclei of these cells would in itself suggest they had ceased to function.

If we were to limit ourselves to the observations on degermed endosperms in water-culture, we should conclude that the diastatic function of the "aleurone-layer" is very small indeed, and this is also apparently confirmed by the impossibility of demonstrating the existence of such a function in the "aleurone-layer" when we employ the methods which have been so successful in this direction in the case of preparations of the scutellum.

When the integuments with the "aleurone-layer" attached are perfectly freed from the starch-containing cells, and are placed face downwards on starch-gelatine, we have never been able to obtain any evidence of action on the starch, and even when the preparation was made so as to include a layer of the amyliferous cells, which were kept moist on gelatine, no influence was exerted on the contained starch. Under these circumstances, however, there is an entire absence of cytohydrolytic as well as of amylohydrolytic action, and since the former is so well marked in degermed endosperms in water-culture, we can only conclude that the separated "aleurone-layer" for some reason or other will not exercise its normal function in the same manner as the scutellar epithelium placed under similar conditions.

It is also to be remembered that in those cases where the endosperm is in actual contact with the embryo, either as in natural germination, or as in the "grafting" experiments, the special changes induced by the "aleuronic" layer proceed much more rapidly than in isolated endosperms in water-culture, and this accelerated action is much more evident in the case of the diastatic than of the cytohydrolytic action.

It would appear, therefore, that although one of the principal functions of the "aleurone-cells" is to break down the cell-membrane of the amyliferous endosperm, these cells also share with the scutellum the power of eroding starch-granules.

Owing to the different method of attack on the starch, it now becomes possible, for the first time, to discriminate one form of action from the other, but it is very difficult to apportion the part played by scutellum and "aleurone-layer" respectively, for the amount of action of either depends not only on the enzymic intensity for equal areas of the two tissues, but also on the total areas facing the endosperm-contents in each case.

Since the total area of the "aleurone-layer" is considerably greater than that of the scutellar epithelium, the influence of the

former may be as great or even greater than that of the scutellum in the early stages of germination, even if its specific enzymic intensity is very much less.

There is another probable function of the "aleurone-layer" which may indirectly be of great value to the seed. These cells, which undoubtedly contain living elements, constitute the outermost peripheral layer of an otherwise *dead* endosperm, which, were it not for this protective sheathing of living cells, would be much more liable to the attacks of any of the micro-organisms of the soil which succeeded in penetrating the seed-envelopes. It is a noteworthy fact that the "aleuronic" cells are much more fully developed over those parts of the seed which may be regarded as devoid of life, and become very much more attenuated where they come into proximity with the embryo whose cells, owing to their activity, do not require an equal amount of protection. In the case of barley the threefold layer of "aleurone-cells" lying within the pericarp and testa constitutes a triple line of defence, which must be of some value in protecting the anyliferous cells against the hordes of external organisms when the grain is placed under the natural conditions suitable for germination.

We must express our great thanks to Mr. W. T. Thiselton Dyer and Dr. D. H. Scott for the opportunities they have afforded us for carrying out this research at the Jodrell Laboratory.

Addendum.

Since writing the above we have for the first time seen the full and expanded account which Puriewitsch has given of his work in Pringsheim's 'Jahrbuch,' vol. 31, 1897, p. 1.

His observations on the self-depletion of the endosperm of the *Gramineæ* take account only of the erosion and dissolution of the reserve starch. He does not call attention to the equally important and necessary antecedent phenomena of cytohydrolysis, which admit of a determination of the "aleuronic" or peripheral origin of the autonomous changes and their mode of progression in the endosperm. Puriewitsch, in fact, regards every cell of the endosperm as capable of functioning as a depletive agent, whereas our own work points strongly to the conclusion that when an endosperm is deprived of its embryo the subsequent chemical changes within it are initiated by the "aleurone-layer" only.

It is correctly stated that such action commences near the scutellar surface, and extends peripherally under the "aleurone-layer"; but the author explains this by the observations of Brown and Morris, and Grüss,* that the proximal half of the endosperm contains more

* The recent experiments of Grüss in this direction were made on maize, not on barley.





FIG. 1.

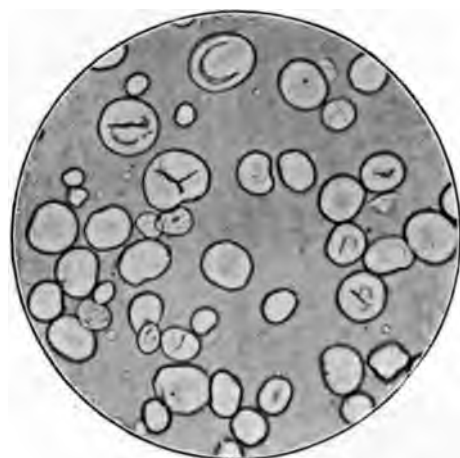


FIG. 2.

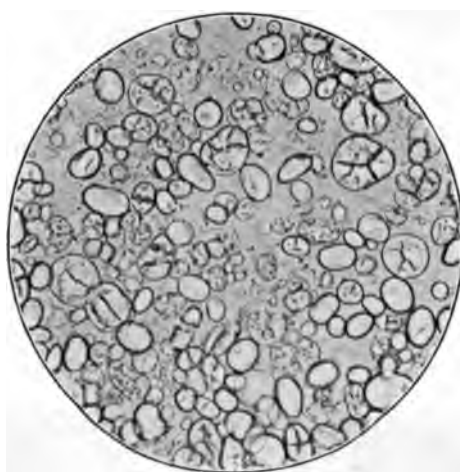


FIG. 3.

diastase than the distal. This cannot be the true explanation, since we find that the pre-existent enzymes of the endosperm practically play no part in the self-depletion.

DESCRIPTION OF PLATE 1.

FIG. 1.—Examples of “sub-scutellar” starch-erosion, showing incipient and advanced forms. Two granules, the one in the centre of the field, the other on the right, show incipient “sub-aleuronic” erosion.

FIG. 2.—Examples of “sub-aleuronic” starch-erosion in incipient stages.

FIG. 3.—Examples of “sub-aleuronic” starch-erosion in more advanced stages.

(For the production of these photographs we are indebted to Mr. Albert Norman.)

March 10, 1898.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Treasurer, in the Chair.

The following Papers were read :—

- I. “On the Rotation of Plane of Polarisation of Electric Waves by a Twisted Structure.” By Professor J. C. BOSE. Communicated by LORD RAYLEIGH, F.R.S.
- II. “On the Production of a “Dark Cross” in the Field of Electro-magnetic Radiation.” By Professor J. C. BOSE. Communicated by LORD RAYLEIGH, F.R.S.
- III. “An Extension of Maxwell’s Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and allied Phenomena.” By EDWIN EUSEB, A.R.C.S. Communicated by Captain ABNEY, F.R.S.
- IV. “On the Relative Retardation between the Components of a Stream of Light produced by the Passage of the Stream through a Crystalline Plate cut in any Direction with respect to the Faces of the Crystal.” By JAMES WALKER, M.A. Communicated by Professor CLIFTON, F.R.S.
- V. “On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force and the Period of Solar Spot Frequency.” By W. ELLIS, F.R.S.

“On the Modifications of the Spectra of Iron and other Substances radiating in a strong Magnetic Field.” By THOMAS PRESTON, M.A. Communicated by Professor GEO. FRANCIS FITZGERALD, F.R.S. Received January 11,—Read January 20, 1898.

Soon after Professor Zeeman announced his important discovery that the spectral lines become sensibly altered in appearance and constitution when the source of light is placed in a strong magnetic field, I determined to examine if the spectra of different substances are equally or similarly affected, and also if the various lines in the spectrum of a single substance are equally or differently affected by the action of the magnetic field. The investigation of these matters was undertaken also with the ulterior object of determining if the phenomenon, for the discovery of which we have to thank Professor Zeeman, could be made to afford any new information, concerning the corresponding sets of lines in homologous spectra or in the spectra of substances belonging to the same group of chemical elements—in fact, to determine if any new information could be gleaned concerning those atomic or molecular vibrations which give rise to the light emitted by incandescent matter, and thence to approach more closely to a knowledge of the ultimate constitution of atomic and molecular structures.

For the purpose of this inquiry I availed myself gladly of the opportunity afforded me of using the excellent Rowland's concave grating mounted in the physical laboratory of the Royal University of Ireland. This instrument is of the usual type, having a radius of 21·5 feet, and ruled with about 14,438 lines to the inch. It is fitted with a camera box which takes a photographic plate 20 inches long and 2½ inches broad, so that a length of the spectrum equal to 19 inches can be photographed at a single exposure. As a consequence I naturally decided to study the effect of the magnetic field on the spectral lines by photography, rather than by eye observation, for the latter, besides being applicable to the visible part alone, is more liable to lend itself to the personal bias or the previously formed opinions of the observer. The photographic plate, on the other hand, does not lend itself to the imagination of the observer, but gives a faithful record of the phenomena as they actually exist in the image focussed upon it; besides it enables one to compare the effects produced on a large number of lines under identical circumstances, a point of great importance.

In the case of a phenomenon which exhibits itself as a very small effect, and one which it is difficult to obtain and observe, it is natural

that some doubt should exist at first as to what it is that actually is presented to the observer; and that discrepancies should occur in the statements of different observers regarding the phenomenon now under consideration is not surprising. Thus while Zeeman distinctly states that he obtained a tripling of the spectral lines when the source of light is viewed across the lines of magnetic force, and a doubling when it is viewed along the lines of force; yet these effects were obtained only after the theory of the phenomena pointed in this direction, and other reliable observers investigating the same lines have given expression to the opinion that when viewed *across* the lines of force a doubling (like a reversal) or a broadening combined with a doubling occurred. So far as I am aware most observers have failed to obtain distinct triplets when the light is viewed across the lines of force, that is, they have not succeeded in separating the constituents sufficiently to enable them to decide what the exact composition of the modified spectral line is. By placing a nicol's prism in the path of the light it has been determined that the central part of the modified line is plane polarised, and that the edges are also plane polarised, but in a plane at right angles to the plane of polarisation of the middle part. Thus with the nicol in one position the middle may be cut out, leaving the two border lines, and with the nicol turned through a right angle the two border lines may be removed, leaving the middle portion alone. It is to be remarked, however, that observations of this nature, although they are in accordance with the supposition that the modified line is a triplet, yet they do not absolutely prove tripling pure and simple. For this purpose it is necessary to obtain a magnetic field strong enough to separate completely the constituent parts of the modified line, so that each can be observed separately, without the complications attending the overlapping of the others.

By way of illustration and explanation of the foregoing remarks it should be mentioned here that the observations which I have made in a general survey of the spectra of several substances indicate the existence of four types of effect,* which, although they easily harmonise as particular cases of one general type of effect, yet deserve to be noted, inasmuch as they are the results actually presented to observation. Thus when the source of light is viewed *across* the lines of magnetic force some of the spectral lines are resolved into sharp triplets, in which the constituent lines are distinctly separated by clear spaces on the photographic plate. In these the middle line of the triplet is stronger than the side lines, and agrees with the expectation that it should contain as much light as the two side lines combined. On the other hand, some lines are not resolved into

* These matters are treated in fuller detail in a paper communicated to the Royal Society in December, 1896.

distinct triplets, but into what one may provisionally term "quartets." In these the two side lines contain nearly all the light, while the two middle lines are weak, but still very distinctly marked. It may be that these arise from a partial reversal of the middle line of the triplet, and that they would consequently show as triplets but for such reversal. The middle line, of course, is the one which would naturally suffer reversal in the layers of colder vapour, as it belongs to the period of vibration which is uninfluenced by the magnetic field—yet the appearance of the weak middle pair is not that which one usually observes in the cases of reversal, nor in any case do the photographs of the line free from the magnetic field show any sign of reversal.

It is interesting to notice in passing that the lines to which attention has been chiefly directed by other observers, namely, the D sodium lines and the blue line of cadmium of wave-length 4800, belong to this latter class, and no doubt the difficulty of deciding as to what really takes place, and the difference of opinion which has existed are to be attributed to this cause. These lines do not resolve themselves into sharp triplets, and if one wishes to observe a distinct and well-defined triplet the violet line of cadmium 4678 or the violet line of zinc 4680 should be chosen. Further, some lines show as doublets, or, if we wish to state it so, as triplets from which the middle line has completely disappeared—the source of light being still viewed *across* the lines of force. Whether this is due to complete absorption of the middle line in the outside vapour of the spark, or not, remains for further investigation. The observation of it, however, led me to the subject of this communication, namely, the investigation of the spectrum of iron. Finally, there are spectral lines which are only slightly broadened, and others which are scarcely affected by the magnetic field, even when this is strong enough to resolve other lines of the same substance into triplets, of which the side lines are more than half a millimetre apart, and visibly resolved on the photographic plate to the naked eye.

Viewed from the theoretical standpoint, we have no reason to demand that all, or indeed necessarily any, of the spectral lines should be resolved into sharp triplets when the source of light is viewed across the lines of magnetic force. For, in order that a spectral line should exhibit itself as a characteristic triplet under the influence of the magnetic field, it is necessary that the freedom of vibration should be equal in all directions, and in this case the intensity of each rectangular component will be the same. Hence the middle line of the triplet will contain as much light as the two side lines taken together. If, however, the vibrations are not equally free in all directions the foregoing result will not hold, and it becomes possible to have a triplet with a weak middle line or with-

out a middle line, in which case it shows as a doublet. For example, if the vibration is restricted to one plane, and if this plane sets itself at right angles to the lines of magnetic force under the influence of the magnetic field, then the component of the vibration in the direction of the lines of force will be zero, and the middle line will vanish from the triplet. If, on the other hand, the whole vibration should set itself parallel to the lines of force, then the side lines of the triplet would vanish, or, in other words, the spectral line would be unaffected by the magnetic field. It is clear, therefore, that the study of the way in which the spectral lines are affected by the magnetic field is likely to throw light on the character of the molecular vibrations.

The substance which one would expect to present peculiarities in this way is iron, which, if it retains any of its magnetic properties at the high temperature of spark produced between the terminals of an induction coil, should exhibit some characteristic behaviour in the magnetic field. I was led to expect, indeed, that many, if not all, of the iron lines would be resolved into doublets rather than triplets when the spark is viewed *across* the field. I found it no easy matter, however, with the magnetic field at my disposal to resolve the iron lines into anything; in the first place, because the effect is much smaller (about one-half) for iron than for the 4678 line of cadmium or zinc, and, in the second place, because it is not possible to work with the pole pieces of the magnet so close together when the spark is passed from a solution of a salt as when it is passed between small metal electrodes. But in the case of a highly magnetic substance like iron the metal becomes almost unmanageable in a powerful magnetic field, and so it happened that in my first attempts I was not successful in resolving the iron lines into either doublets or triplets; yet even at this stage I observed two or three lines converted into what I considered doublets. Finally, I succeeded in resolving the vast majority of the lines of iron by enclosing pieces of iron wire in small glass tubes so that the ends of the wire protruded slightly from the glass where the spark occurred. The pole pieces were then pushed up to touch the glass jackets, and the spark was thus obtained in a very strong field, in fact in a field strong enough to resolve the majority of the iron lines into distinct triplets, showing that these vibrations possess freedom in the magnetic field (at the temperature of the spark); but, in addition to this, other lines are observed as distinct doublets, and the appearance of these doublets is not that usually associated with a reversal. In addition there are other lines in the spectrum of iron which are scarcely affected, if at all.

It is important to remark that these differences of effect by the same magnetic field on the different lines of the spectrum of the same substance are *not shown in any particular ascending or descending*

30 *On the Spectra of Iron, &c., radiating in a Magnetic Field.*

order in the spectrum. They may all exist in a single group of lines of nearly the same wave-length, and the magnitude of effect, even in the case of the lines which triple, cannot at present be stated to follow any law. Such a law, if one exists, can be discovered only by a complete survey of the spectra of various substances with a very powerful magnetic field capable of wide limits of variation. Such a field I hope to possess in an electromagnet which is being built for the purposes of further inquiry in this direction.

In the meantime it may be taken as thoroughly determined that it is untrue to assume that there is any such law as that the effect is uniform for the various spectral lines of any substance, or that the effect varies as the wave-length or as the square of the wave-length. No doubt it may be possible in the future to divide the spectral lines into groups such that the members of each group are similarly affected; and it may be that a certain homology may be found to exist between the groups of one substance and those of other substances, and hence the effect produced by the magnetic field may be brought into line with those chemical and physical properties of the elements which have already thrown them into periodic groups.

I am at present very hopeful that some law will be obtained in this department for the phenomena attending the action of a magnetic field on the radiation from a source placed in it, for the results which I have already obtained show certain correspondences in the effects produced on the spectra of allied elements as well as certain recurrences of the same effect in the spectrum of the same substance.

The amount of ground so far covered has not, however, been sufficient, nor indeed has my magnetic field been sufficiently strong, to enable me to lay hold of any general principle controlling the observed effects; nevertheless a useful purpose will be served in publishing this preliminary statement, as it will, at least to some extent, clear the ground for those who are at present considering the phenomena from a purely theoretical standpoint, and it may also to some extent serve a perhaps equally useful purpose in diverting a useless expenditure of energy on the part of those who may be engaged in formulating the laws of the phenomena without sufficient knowledge of the facts.

In conclusion I may mention that the spectral lines of air and hydrogen are rendered diffuse by the action of the magnetic field. The broadening here is not of the character exhibited by spectral lines of metals in which the lines remain sharp though easily broadened, but it is more of the character of a broadening produced by increase of pressure of the gas.

I may also add that the magnetic effect does not appear to be in any way parallel with the pressure effect, that is, the displacement of *the lines caused by increase of pressure in the gas surrounding the*

source of light, which has been recently investigated by Messrs. Humphreys and Mohler.

The photographs referred to in this communication were taken with a spark source of light placed between the pole pieces of a good electromagnet of the ordinary U-shape, for the use of which I am indebted to the kindness of the Right Rev. Monsignor Molloy. With this magnet I was able to obtain a field of about 25,000 C.G.S. units, and this separated the side lines of the triplets in the violet line 4678 of cadmium to a distance of 0.56 mm. apart. The same separation takes place in the triplet given by the violet line 4680 of zinc, so that in the case of these lines we may infer in round numbers that a magnetic field of 20,000 C.G.S. units strength produces a difference of wave-length of one Ångström unit between the side lines of the triplet, i.e., a separation equal to one-sixth the difference of wave-length between the two D lines of sodium. For iron the separation is about half this amount in the case of several lines.

“Note on the Connection between the Faraday Rotation of Plane of Polarisation and the Zeeman Change of Frequency of Light Vibrations in a Magnetic Field.” By GEO. FRAS. FITZGERALD, F.R.S., F.T.C.D. Received March 2, 1898.

(Being Notes of a Contribution to the Discussion of Mr. Preston's Paper above, read January 20, 1898.)

The rotation of the plane of polarisation of light in a magnetic field is due to the velocity of propagation of light circularly polarised in one direction being different from that of light circularly polarised in the opposite direction. The Zeeman effect is due to a difference in the frequency of vibration of these circularly polarised waves. What is required is to connect the frequency of vibration with the velocity of propagation.*

All modern theories of dispersion connect these two quantities. The velocity of propagation of light in transparent media is now universally considered to be determined in part by what may be

* From an abstract of a paper by M. Becquerel, in the ‘Comptes Rendus’ of last year, I understand him to view the Faraday effect as due to a carrying round of the light vibrations by matter rotations. This is quite in accordance with the view sometimes held as to refraction, namely, that it is due to the waves being propagated through the molecules more slowly than through the ether. The dynamical theory of these views is difficult on account of the smallness of the molecules in comparison with the length of the waves. M. Becquerel's view, as I understand it, makes the Faraday effect depend on a change of frequency of rotation of the waves in matter rather than on their velocity of propagation, and is consequently quite at variance both with the commonly received theory and with the one put forward in this note.

called the syntony of the matter and light vibrations, and is consequently dependent on the frequency of the matter vibrations. In most substances the dispersion is controlled within the visible spectrum by a great absorption band in the ultra-violet, this band representing a possible frequency of vibration of the molecules, *i.e.*, of that part of the molecules which affects the ether, be it electrons or something which simulates the actions ascribed to electrons. If owing to any cause this absorption band be changed in position, *i.e.*, the frequency of the molecular vibrations be altered, the dispersion of the medium will be changed, and with it the velocity of propagation of light within the visible spectrum. Now Zeeman has shown that in a magnetic field the frequency of vibration of molecules *producing* light circularly polarised in one direction is different from that of molecules *producing* light circularly polarised in the opposite direction, and that consequently the absorption bands for molecules in a magnetic field for oppositely circularly polarised waves will differ. Hence we conclude that the velocity of propagation of oppositely polarised waves within the visible spectrum will differ, and that is the Faraday effect. Hence these two phenomena are directly connected with one another, independently of any other than general dispersion theory, *i.e.*, independently of any theory of electrons, such as Lorentz has shown will explain the Zeeman effect. If we introduce such a theory, and use it to explain the Faraday effect upon the lines now laid down, we arrive at the interesting conclusion that before the Zeeman effect was observed the Faraday effect would have shown that in the majority of substances we must assume the ether vibrations to be due to the motion of a *negative* electron. In substances with a negative Faraday effect, such as some magnetic bodies, we may conclude either (a) the ether vibrations are due to a *positive* electron, or (b) the absorption band controlling the dispersion is in the ultra-red, or (c) the cause of the Faraday effect may be due rather to a difference of *intensity* of the absorption band for oppositely circularly polarised vibrations than to a difference of *frequency*. This latter alternative is in some ways the most consonant with the usual theory as to the difference between paramagnetic and diamagnetic bodies.

If we proceed to calculate what amount of Faraday effect might be expected from the observed value of the Zeeman effect, we are met by the difficulty of obtaining data. We require to know, for some one or more substances, the amount of the Zeeman effect, the amount of the Faraday effect, and the ordinary dispersion of the substance for light. The only gas (and it is only in gases that the Zeeman effect has been observed) for which the dispersion and Faraday effect are well ascertained, is air, and for it the amount of *the Zeeman effect* has not been published. In preparation, however,

for the data, it may be interesting to write down shortly the equations which, upon Lorentz's theory as to the cause of the Zeeman effect and Larmor's theory as to dispersion, we may expect to hold.*

Assuming, then, that the electric displacement in the medium is partly due to the electric force and partly to the displacement of electrons, we may write for its components in the wave face supposed plane and perpendicular to $z = 0$,

$$f = KP + ex, \quad g = KQ + ey,$$

where P, Q are electric force, e the electron, and x, y are coordinates that measure the displacement of this latter. From these and the usual equations connecting magnetic and electric force we get for the medium

$$K\ddot{P} + e\ddot{x} = \frac{d^2P}{dx^2}, \quad K\ddot{Q} + e\ddot{y} = \frac{d^2Q}{dx^2}.$$

For the motion of the matter, if m be the mass of whatever moves with the electron, matter or effective inertia of ether, and H be the component of the magnetic force normal to the wave, and k the coefficient of restitution of the matter displacement which controls its free period,

$$m\ddot{x} + kx = eP + eH\dot{y}, \quad m\ddot{y} + ky = eQ - eH\dot{x}.$$

It is to be observed that if we assume the motion periodic these equations can be reduced to the form that Drude† and Leathem‡ have shown to lead to results that agree with the observations on the effects of magnetised media on the transmission and reflection of light.

If we substitute in these equations what obviously solves them, the equations of a right- or left-handed circularly polarised wave of frequency $n = p/2\pi$, and wave-length $\lambda = 2\pi/q$, and whose amplitude in x and y is a , and in P and Q is A , we get

$$(KA + ea)p^2 = Aq^2,$$

$$a(mp^2 \pm eH \cdot p - k) + eA = 0.$$

If we substitute for $k = mp_0^2$, where p_0 is a measure of the frequency of the free period corresponding to the forces kx, ky which have been assumed above to control the motion of the electron, we get for the velocity of propagation $p/q = V$,

$$V^{-2} = K + e \frac{a}{A} \quad \text{and} \quad \frac{a}{A} = \frac{-e}{m(p^2 - p_0^2) \pm eHp}.$$

* Larmor, 'Phil. Trans.,' A, 1897.

† 'Wied. Ann.,' 1896.

‡ 'Phil. Trans.,' A, 1897.

These enable us to calculate the dispersion of the substance in terms of the difference of velocity of propagation of waves of oppositely circularly polarised light.

To get approximate formulæ in terms of quantities that can be observed, we have the refractive index $\mu = V_0/V$, when V_0 is the velocity in *vacuo* and $p = 2\pi V/\lambda$. Assuming that λ_0/λ is small, i.e., that the dispersion is due to an absorption band far up in the ultra-violet, we get, writing ρ for e/m ,

$$\mu^2 = \mu_0^2 + \frac{e\rho\lambda_0^4}{4\pi^2\lambda^2} \pm \frac{e\rho^2\lambda_0^4}{8\pi^2V\lambda} H.$$

The second term on the right-hand side of this equation gives the ordinary dispersion, while the third term gives the Faraday rotation. The first term μ_0 is given by

$$\mu_0^2 = KV_0^2 + \frac{e\rho\lambda_0^2}{4\pi^2},$$

and is, as Mr. Larmor has pointed out, composed of two parts, the first being essentially refraction and the second dispersion.

In the case of air, it is possible to compare this equation roughly with observation. The equation is of the form

$$\mu^2 = \mu_0^2 + \frac{a}{\lambda^2} \pm \frac{a}{\lambda} \cdot \frac{\rho H}{2\pi V_0}.$$

We may estimate a from the dispersion in air, and it is approximately 1.8×10^{-14} .

The equation gives for the two absorption bands that exist instead of $p = p_0$,

$$p_0^2 - p_1^2 = \rho p_1 \cdot H, \quad p_0^2 - p_2^2 = \rho p_2 \cdot H, \\ \therefore p_2 - p_1 = \rho \cdot H.$$

So that, if δp be the difference of frequency for unit magnetic force,

$$\delta p_0 = \rho.$$

Hence, for the difference of refractive index of two circularly polarised rays, we have

$$\mu\delta\mu = \frac{a}{\lambda} \cdot \frac{\rho H}{2\pi V_0} = \frac{a}{\lambda} \cdot \frac{\delta p_0}{2\pi V_0} \cdot H.$$

Assuming, what is certainly not accurately true, that ρ is the same for all lines, and taking that Mr. Preston's estimate for some lines of Zn applies to oxygen, namely, that 1 A.U. change of wave-length is produced by a field of 20,000 C.G.S. units of magnetic force, we get—

$$\delta\mu = 4 \times 10^{-14}.$$

The observed rotation in oxygen gives a value for this of 3.5×10^{-14} , so that it is about equal to the calculated value.

The discrepancy may be due to the fact that ρ is not the same for all spectral lines, and we could not reasonably expect to get an accurate result by assuming that the Zeeman effect in zinc can be used in calculating the Faraday effect in oxygen. On the whole I think the calculation shows that what must be a *vera causa* for a Faraday effect is the whole cause of it. From the mere fact that rotatory polarisation is approximately inversely as the square of the wave-length and consequently vanishes for long waves, it follows that it is essentially a dispersion phenomenon.

“On artificial temporary Colour-blindness, with an Examination of the Colour Sensations of 109 Persons.” By GEORGE J. BURCH, M.A. Communicated by Professor GOTCH, F.R.S. Received February 5,—Read February 17, 1898.

(Abstract.)

By exposing the eye for a sufficient length of time to bright sunlight in the focus of a burning glass behind suitably chosen transparent screens, it is possible to induce over the whole retina a condition of temporary colour-blindness.

After red light the observer is for some minutes completely red-blind, so that scarlet geraniums appear black and roses blue, while yellow flowers seem various shades of green, and purple flowers look violet. The same mistakes are made in sorting Holmgren's wools as by the red-blind.

Temporary violet blindness may be brought about by using a tank of ammonio-sulphate of copper. While it lasts, violet wools look black and purple flowers crimson, but the green foliage appears of a richer tint than usual. The recovery from violet blindness is very slow.

Green blindness may be brought about by exposing the eye to light through three thicknesses of green glass. The colour-scheme of the landscape during this condition is that of a picture painted with vermilion, flake-white, and ultramarine, variously blended.

Purple blindness may be produced by a combination of films stained with magenta and aniline violet, by which the green is absorbed. During purple blindness the vision is practically monochromatic, no colour being visible but green.

If one eye is rendered purple-blind and the other green-blind, the observer sees all objects in their natural colours but with a curiously exaggerated perspective due to the difficulty of combining the images perceived by one eye with those visible to the other.

General Appearance of the Spectrum during Temporary Colour Blindness.

A large spectroscope, in which only about a tenth part of the spectrum is visible at once, is directed to the sun, and the slit opened till the illumination is as intense as can be borne. After the eye has been sufficiently fatigued the results are observed in a single-prism spectroscope in which the entire spectrum is visible.

The following parts of the spectrum, namely, the red from A to B, the green from the neighbourhood of E, the blue about half-way between F and G, and the violet at and beyond H, produce well-defined and characteristic results, whereas the intermediate portions of the spectrum produce results intermediate in character.

That is to say, while exposure to red light causes changes affecting the red, and exposure to green light produces corresponding changes in the green, yellow light, instead of causing corresponding changes in the yellow, affects the whole of the red and the whole of the green, the total change being equal to the sum of the changes due to excitation by red light and by green light separately.

The effects produced by each of the four above-mentioned colours differ in degree but not in kind.

(1) In each case all direct sensation of the colour used for fatiguing the eye is lost.

(2) There is produced a positive after-effect of the same colour by which the hue of all other colours is modified if they are relatively weak, but which is unnoticed if they are bright.

(3) The temporary abolition of any one colour sensation is without effect on the intensity of the remaining colour sensations.

(4) Any two or any three of these four colour sensations can be simultaneously or successively exhausted.

(5) The positive after-effect of red is very transient; that of green lasts longer and is more noticeable; that of blue is still more powerful and persistent; and that of violet is strongest and lasts a long while. As the positive after-effect subsides the colour sensation returns, but the positive after-effect becomes unnoticeable long before the colour sensation is restored to its full strength.

(6) During the process of dazzling the eye the observer is conscious of the progress of the change, but only realises the extent of his colour blindness on attempting to examine a less brilliantly illuminated spectrum.

The positive after-effect does not in these experiments pass through cyclic changes of tint as after-images appear to do under other conditions.

Examination of the Phenomena with a Spectroscope of Wide Dispersion.

Green blindness was produced by the spectroscopic method already described. When the exposure was complete the slit was closed until the Fraunhofer lines were sharply defined. All sensation to green was lost, the red appearing to meet the blue in the centre of the field. The position of the junction of these two colours could be varied considerably by exposing the eye to strong red or strong blue light, thus showing that the red and blue overlap. But violet light had no effect upon the position of the junction of red with blue.

Similarly, during blue-blindness, the green and violet were seen to overlap, exposure to green light shifting the junction towards the green and *vice versa*. Red light had no effect on the position of the junction of green with violet during blue-blindness.

The phenomenon of flickering* visible between the red and the green of a highly magnified spectrum, is also seen at the junction of red with blue during green-blindness, and of green with violet during blue-blindness, as well as at the junctions of green with blue and of blue with violet under normal conditions.

The author has succeeded by an exposure of three minutes to light from between H and K in blinding the eye to violet without affecting the blue, the real hue of which is thus seen, unaccompanied by any other colour sensation.

These experiments lead to the conclusion that no one colour-sensation is related to any other in the sense indicated by Hering. Each may be exhausted without either weakening or strengthening the others. The observed facts are, in the author's opinion, more in accordance with the Young-Helmholtz theory, but they imply the existence of a fourth colour sensation, namely, blue.

Examination of the Colour Sensations of 109 Persons.

The tests employed were Holmgren's wools, supplemented by gelatine films stained with various colours, Hering's method of coloured shadows, and the author's spectroscopic method, which was applied to seventy normal cases in the following manner.

Using the large spectroscope referred to, with the slit narrow so as to give a comfortable degree of illumination, the observer selects those portions of the spectrum at which he sees a marked change of hue. He then looks at the red between A and B for thirty seconds, and at a given signal traverses the spectrum rapidly, stopping at the first of these changes.

Next he looks at the green for thirty seconds before turning to the

* 'Physiol. Soc. Proc.,' June, 1897.

second change of hue. Again, after looking at blue for thirty seconds he seeks the third change of hue. The next step is to trace the violet to its limits. After this he works through the spectrum back again, fatiguing the eye with violet before finding the blue, and so on, ending with the determination of the limits of the red. The degree of fatigue is so slight that he is quite unconscious of it.

The seventy cases examined in this way agree as to the number and mean position of the changes of hue, but they may be divided broadly into those whose colour sensations overlap and those whose colour sensations do not overlap, *i.e.*, those who find the changes of tint occur in the same place when working from red to violet as when returning from violet to red.

The first class includes persons both educated and uneducated whose avocations require them to compare colours. The second comprises all who fail with the closer shades of Holmgren's wools. Details are given of some in whom the green and violet are so far extended into each other that they see practically no pure blue, and it is suggested that these, and other differences in the relative intensity and extent of the colour sensations may account for the divergence of opinions among writers on the subject. The paper concludes with an account of five cases of red-blindness.

"On the Connection between the Electrical Properties and the Chemical Composition of different kinds of Glass."

By Professor ANDREW GRAY, LL.D., F.R.S., and Professor J. J. DOBBIE, M.A., D.Sc. Received February 7,—Read February 17, 1898.

The experiments and results described in the following paper are a first instalment of work we have undertaken with a view to finally determining, if possible, the circumstances which affect the conductivity and specific inductive capacity of glass. It appeared from some experiments which were carried out by Professor T. Gray and ourselves some years ago,* that it might be of interest to have a number of glasses specially made up with a view to testing some of the conclusions then arrived at.

A result previously obtained by Professor T. Gray had shown that potash and soda lime glasses have a higher conductivity than flint glasses; this result had also been arrived at by Dr. Hopkinson. In particular it seemed desirable to ascertain whether by increasing the amount of lead oxide and diminishing the amount of soda, the conductivity would go on diminishing. We have experienced great

* 'Roy. Soc. Proc.,' No. 231, 1884.

difficulty in getting glasses made according to our own specifications. We endeavoured to make the glasses ourselves, and several experiments were made accordingly, both in the laboratories here and at the Ogwen Tile Works, where a large furnace had been erected for the construction of tiles from slate dust. Some success was achieved, but it was found impossible, without the expenditure of far more time than could be spared, to obtain the glasses in a condition suitable for the experiments we wished to carry out.

Through the kindness, however, of Messrs. Schott & Co., of Jena, and of Messrs. Powell & Sons, Whitefriars, London, we have recently obtained a number of specimens of glass all richer in lead than the specimens formerly available, and, further, in some cases practically free from soda. We have also had made to order by Messrs. Schott specimens of their own glass, used, we believe, chiefly in the construction of thermometers, as well as of a barium crown glass, which have not hitherto, so far as we are aware, been experimented with.

Determination of Conductivity.—The method of experimenting followed was practically the same as that described in the paper already referred to, but its nature may perhaps here be indicated.

Owing to the large percentage of lead oxide in some of the glasses prepared for us by Messrs. Schott, it was found impossible to blow them into flasks, and they were therefore cast into plates; the arrangements therefore required some modification for their case.

The specimens which were in the form of flasks were filled up with mercury to the bottom of the stem (which in most cases was about 8 or 9 inches long), and the flask thus filled was sunk in a bath also containing mercury, so that the mercury was at the same level inside and outside. One terminal of a circuit containing a battery of about thirty secondary cells and a very sensitive galvanometer was connected to the mercury within the flask by a wire passing down the neck, while the other terminal was connected to the mercury in the bath.

The galvanometer was the instrument formerly used and described in 'Roy. Soc. Proc.,' vol. 36, p. 287. It was carefully insulated, as was also the reversing key, and all necessary precautions were taken to make sure that the current passing through the galvanometer was that passing through the walls of the flask between the mercury coatings. Thus it was always verified that no deflection took place when the wire was withdrawn from the flask and placed round the outside of the neck. This test obviated the possibility of the existence of any disturbing film of moisture on the surface of the glass. The bath could be heated to any temperature required in the experiments.

The conductivity was calculated from deflection of the galvano-

meter produced by the current through the glass, the area and thickness of part of the specimen in which the current flowed, as described in the former paper.

In the capacity measurements the plate or flask, as the case might be, was supported as described above. A quadrant electrometer was kept connected to the plates of one of Lord Kelvin's air leydens. This was charged with twelve secondary cells, and therefore to a difference of potential of about 24 volts. After the battery had been removed a reading was taken of the electrometer deflection, and then the specimen was connected for a very short interval of time as a condenser in parallel with the leyden.

This connection was made by means of a myograph pendulum which, when freed, swung over a considerable arc to a catch which prevented it from returning. At its lowest point a metal piece projecting below the bob touched the top of a tongue projecting upwards from a hinge at its lower end, and leaning against the point of an adjustable screw. The connection between the two condensers thus only endured while the three pieces, the screw, tongue, and bob, were in contact. This was only the interval of time required for a pulse of flexure to travel about a centimetre in a bar of steel about half a centimetre thick, and about a centimetre broad. The interval was reckoned as at most about $1/30,000$ of a second.

The plates were originally rather over a quarter of an inch thick, and after some observations of capacity had been made on some of them, and it had appeared that their resistance was too great to be measurable, they were cut down on a turning-table used for cutting slates, to a thickness of about 3 mm.; they were then fixed on a bed of pitch, and ground down by hand to a thickness of about 0.24 cm. They were polished and properly cleaned, and then covered on both sides with a dense and thoroughly adherent coating of silver. This was cut away for a space of about half an inch round the edges. Great care was taken to remove every trace of silver, and to make the edge thoroughly clean.

While being experimented on, the glass plate was laid with one coating of silver resting on a plate of copper at the bottom of an iron bath. Another plate of copper was laid on the upper coating of silver, and kept down with a weight, and the connections of the battery circuit, described above, were made with the copper plates. The iron bath was placed within a larger bath partly filled with sand, so that the temperature could be raised by heating the outer bath from below.

The results of the experiments are exhibited in the table which follows. We have there given the density, specific resistance, specific inductive capacity, and chemical composition of each specimen experimented on.

No.	Description of glass.	Density.	Specific resistance (ohms).	Specific inductive capacity.	Silica.	Lead oxide.	Ferro oxide and alumina.	Arsenious oxide.	Potassium oxide.	Sodium oxide.	Manganese monoxide.	Remarks.
XXI	Lead potash glass, made by Messrs. Powell and Sons, London.	3.495	Too high to measure, certainly above $18,000 \times 10^{10}$ at 130°C .	7.966 at 15°C . 7.930 at 120°C .	50.5	42.14	0.41	...	6.93	trace	trace	Absorption effects in electrical experiments very marked.
XXII	Lead potash glass, made by Messrs. Schott and Co., Jena.	3.591	Too high to measure, certainly above $35,000 \times 10^{10}$ at all temperatures to 135°C .	7.991 at 14°C .	44.5	46.6	trace	0.4	8.0	0.5	trace	Absorption effect slight.
XXIII	Barium glass, made by Messrs. Schott and Co., Jena.	3.565	Too high to measure, certainly above $59,000 \times 10^{10}$ at all temperatures to 140°C .	8.5. No variation with temperature. No absorption effect.	33.0	12.0	6.0	1.0	...	43.0	...	This glass showed no trace of polarization. Trace of arsenious oxide.
XXIV	Zinc soda glass, made by Messrs. Schott and Co.	3.493	508.5×10^{10} at 45°C . 0.200×10^{10} at 140°C .	7.51 at 15°C . Conduction too great at high temperatures.	67.6	8.0	trace	0.4	9.0	...	5.0	Considerable absorption effects.

XXI. Flask. XXII. Plate. XXIII. Plate. XXIV. Flask.

Internal radius 3.741 cm. Length of silvered surface 19.81 cm. Length of silvered surface 18.3 cm. Effective area 209.2 sq. cm.

External " 3.532 " Breadth of ditto 11.90 " Breadth of ditto 14.31 " Mean thickness 0.935 cm.

Effective surface 174.921 sq. cm. Area 185.439 sq. cm. Area 249.028 sq. cm. Mean thickness 0.724 cm. Mean thickness 0.935 cm.

Experiments were made with four different flasks of this glass, with results fairly represented by those given in table. The factor of diminution of resistance for a rise of temperature of 20°C . was about one-fifth.

The resistance was taken after five minutes' electrification in each case. The "Jena" glass (XXIV of the table), in both resistance and capacity experiments, showed very considerable effects of dielectric polarisation, which were a very long time in disappearing, though the conducting coatings of the flask were kept short-circuited. The dielectric polarisation of the lead glass made by Messrs. Powell was also considerable.

On the other hand it is very remarkable that the barium glasses XXIII, in the capacity experiments, showed little or no sign of polarisation effects; and we propose to make some further investigations of the physical properties of this glass.

In our previous paper the results obtained with eight different samples of lead glass were compared, and it was shown that the electrical conductivity fell off almost quite regularly as the amount of lead oxide increased, and increased with an increase in the amount of soda. The glass which possessed the highest specific resistance (8400×10^{10} ohms) contained 40.5 per cent. of lead oxide, 7.5 per cent. of potassium oxide, and 2.1 per cent. of sodium oxide. Both of the lead glasses dealt with in this paper contained a still higher percentage of lead oxide, and were almost free from soda, and the electrical resistance was so great as not to be measurable. It is, of course, impossible to say how far this result is due to the increase of lead oxide, and how far to the elimination of soda. With the view of definitely settling this point, Messrs. Powell and Sons have kindly undertaken to prepare for us a glass exactly similar to XXI, but having the potash replaced as nearly as possible by the equivalent amount of soda. It should be noticed that the amount of foreign matter (i.e., of ingredients other than silica, lead oxide, and alkali) present in glasses XXI and XXII is very small, and is less than one-fourth of the amount present in the purest glass previously tested, which was also, it may be mentioned, the glass possessing the highest resistance.

It is noteworthy that the barium glass XXIII has a very high resistance, and in this respect behaves more like lead than lime glasses, which have usually a low resistance. It is impossible, however, in view of the somewhat complicated composition of this glass, to say how far the high resistance is due to the presence of the barium, and how far it may be influenced by other ingredients, especially the boric acid, which was not present in any of the glasses previously examined by ourselves or others.

The "Jena" glass XXIV has a low resistance, as was to be anticipated, from its high percentage of soda and complex composition.

The chemical composition of glass XXI is capable of being expressed with tolerable accuracy by a simple chemical formula, and *this* is also in accord with previous experience, which pointed to the

conclusion that a glass, which approaches in composition to a definite chemical compound, has a high resistance.

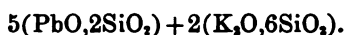
Our knowledge of the chemistry of glasses is still very imperfect, and we cannot say in what way the silica is distributed amongst the bases. We give, therefore, merely the relative number of formula weights of each oxide present, calculated from the analytical numbers, after allowing for the elimination of traces of foreign matter.

Specimen XXI.

After allowing for traces of iron and alumina, the composition of this glass may be expressed by the formula



or



	Found.	Calculated.
SiO ₂	50.72	50.53
PbO	42.32	42.33
K ₂ O	6.96	7.13
	<hr/> 100.00	<hr/> 99.99

Specimen XXII.

This glass is essentially a lead potash glass mixed with a very small quantity of a lead soda glass. Allowing for the small quantity of manganese oxide, arsenious oxide, and other impurities present, and eliminating the soda and a corresponding quantity of silica, calculated on the assumption that each formula weight of soda is in combination with three of silica, we obtain as an expression for the composition of the lead potash glass



	Found.	Calculated.
SiO ₂	44.07	44.11
PbO	47.72	47.82
K ₂ O	8.19	8.06
	<hr/> 99.98	<hr/> 99.99

Specimen XXIII.

This is a borosilicate of barium and alumina. After allowing for the small quantities of arsenious oxide and manganese oxide which it contains, it has the composition



	Found.	Calculated.
SiO ₂	33·33	33·35
BaO	48·48	48·20
Al ₂ O ₃	6·06	6·02
B ₂ O ₃	12·12	12·40
	<hr/> 99·99	<hr/> 99·97

Specimen XXIV.

The "Jena" glass is essentially a borosilicate of zinc, magnesia, containing



	Found.	Calculated.
SiO ₂	67·87	68·05
ZnO	9·03	9·11
Na ₂ O	10·04	10·46
MgO	5·02	4·50
B ₂ O ₃	8·03	7·87
	<hr/> 99·99	<hr/> 99·99

"On the Magnetic Deformation of Nickel." By E. JONES, D.Sc. Communicated by Professor ANDRI F.R.S. Received February 8,—Read February 17

On a former occasion a paper was communicated to the Society* containing an account of some measurements of magnetic contraction of a nickel wire, and a comparison of the values deduced from Kirchhoff's theory. It was then that the most important term in the calculated value of the contraction of a long wire of soft magnetic metal is represented by $\frac{1}{2}H(\delta I/\delta P)$, where H is the magnetising field and δI the increase of magnetisation produced by a small increase of longitudinal field δP per unit area. The results showed that the observed contraction in nickel was much greater than the calculated value. It was sought to establish an empirical equation which might represent the observed effects, and it was found that the difference between the observed and calculated contraction was approximately proportional to the fourth power of the magnetisation.

It was suggested to me at the time that this result might be tested by repeating the experiments under different conditions in order to find out whether it was generally true.

* 'Phil. Trans.,' A, vol. 189 (1897), p. 189.

In offering the following results, I wish again to say that the measurements were made in the Physical Laboratory of the University College of North Wales, and that I am greatly indebted to Professor A. Gray for providing me with the apparatus, and allowing me the time necessary for the experiments, and for many valuable suggestions.

Preliminary experiments were first made to find the nature of the influence of temperature on the magnetic contraction. The apparatus was essentially the same as described in the former paper, with the addition of a spiral tube and burner for heating the water just before it entered the coil. The temperature of the water on entering and leaving the coil was indicated by two thermometers inserted in capsules through which the water flowed. When the water was warmed and allowed to flow for some time, the two thermometers indicated steady temperatures differing generally by about 2° C.

The change of length was magnified by the same lever arrangement as before, and observed by a telescope, scale, and mirror. A few trials showed that it would be impossible to make any measurements when the water was warmed unless precautions were taken to remove air dissolved in the water, for this, on being liberated in the heater, ascended in bubbles through the coil and disturbed the apparatus so much that no readings could be taken. To prevent this, the water was made to flow into a vessel through a pipe near the top, and escape by a pipe near the bottom, whence it proceeded to the spiral heater. A tall glass tube projected upwards from the vessel, and the vessel was heated by a Bunsen flame. The dissolved gases were thus liberated in this vessel, and, collecting at the top, escaped at intervals up the glass tube. By this means the apparatus was rendered steady enough to admit of readings at moderately high temperatures. Further, by regulating the supply the water could be made to stand at any level in the glass tube. Thus the flow of water through the coil, and hence the temperature of the coil could be controlled.

The glass tube was about 6 feet long, and by raising the level of water in it through 1 foot the temperature of the coil was lowered by about 1° C.

Observations of the change of length were always made at night, generally between 10 P.M. and 2 A.M., the apparatus never being sufficiently steady in the daytime.

The nickel wire was the same as that used in the former experiments, of length 83.4 cm. between the terminal brass pieces, and mean diameter 1.65 mm. This was re-annealed before the present measurements.

A number of observations were then made of the change of length

and corresponding field strength on several nights in July and August, 1897, the temperature indicated by the coil thermometers being about 20°C . As before, the temporary and residual contractions were measured separately, the total contraction corresponding to any field strength being obtained as the sum of the temporary effect (observed as an elongation when the current was broken), and the residual effect observed by first demagnetising the wire by reversals, then quickly making and breaking the current. It was found that the curves thus obtained were not the same on different nights, but showed a continual diminution of contraction as time went on. This is probably due to a slow hardening of the metal which seems to go on for some time after the wire has been annealed. The effect of this change can be seen by comparing Tables I (column 2) and III (column 4), which contain values of the contraction of the wire in millionths of its length for a series of fields, observed about the middle of August, 1897, and at the end of December, 1897, respectively.* The wire was annealed on July 3, 1897.

In order to observe the effect of a change of temperature on the magnetic contraction at any field strength it was necessary to take readings at two temperatures immediately after one another, so that the results would not be affected by the above-mentioned time effect. This was done on several nights and with several field strengths, the two temperatures used being about 20°C . and 55°C . The results showed that at low fields (up to about 90 C.G.S.) this rise of temperature of 35°C . caused an increase, at higher fields a diminution, of contraction; and if the temperature was then lowered back to 20°C . the contraction returned to its former value.

Table I contains values of the change of length in millionths and corresponding field strength at the two temperatures, determined on two successive nights, the effect at both temperatures being observed at the lower fields on one night, and at the higher fields on the next. Each field was reversed several times before readings were taken, and the wire was demagnetised on each night while the temperature was being changed. The curves representing these results (contraction, field) have the same general form as was described in the former paper, but the temporary contraction is here considerably greater, though the residual contraction is much the same as before. The effect of temperature is chiefly seen in the temporary contraction: the residual effect, however, appears to be slightly less at the higher temperature at all fields.

On referring to Professor Ewing's 'Magnetic Induction in Iron and other Metals' (§ 114, p. 169), I find that rise of temperature causes increase of magnetisation in nickel at low fields and diminu-

* The temperatures on the two occasions differed by about 10°C ., but the effect of this would be comparatively small.

tion at high fields. Change of temperature, therefore, seems to have similar effects on the magnetisation and contraction curves. Of course this comparatively small difference of temperature, 35° C., would have but a small effect on the magnetisation curve, but as the contraction seems to depend on rather high powers of the magnetisation, it might be expected that the effect would be more noticeable in the change of length than in the magnetisation.

Measurements were next made of the magnetisation and the effect of change of tension on the magnetisation, in order to calculate the quantity

$$\frac{0.123}{10^{11}} \cdot I^2 + \frac{0.00587}{10^{11}} IH + \frac{1}{2} H \frac{\delta I}{\delta P} \dots\dots\dots (1),$$

which was shown in the former paper to be the value of the elongation, $\delta l/l$, of the wire deduced from Kirchhoff's theory. For these measurements a coil of 611 turns of No. 40 double silk-covered and shellacked copper wire was wound on the nickel wire near the middle, and connected in series with a ballistic galvanometer, and with the secondary of a standardising solenoid. The magnetisation was determined by reversing a measured magnetising current, and the galvanometer standardised by reversing a current in the solenoid, the deflections being observed in the two cases. The galvanometer was standardised before and after each set of readings.

Since the resistance in circuit with the galvanometer changed when the temperature of the coil on the nickel wire changed, care was taken that the temperature indicated by the coil thermometers was the same in the magnetisation and standardising experiments. By using a series of different currents in the standardising coil, it was verified that the "quantity of electricity" flowing through the galvanometer was proportional to the sine of half the deflection of the needle. The deflections were observed by a telescope and scale at a distance of 124 cm.

In observing the influence of tension on magnetisation, galvanometer readings were taken first with a load of 1.4 kg. on the wire, then after an additional weight of 7 kg. was applied. The magnetising current was reversed several times and the load applied and removed several times before readings were taken. This was repeated for various field strengths, ranging between 30 and 320 C.G.S., and with the coil at 10° C. and 55° C. Then the magnetisation curve of increasing reversals was determined for the mean load 4.9 kg. at both temperatures, after which the coil was removed from the nickel wire, and observations made with load 4.9 kg., and at the same two temperatures, of the magnetic contraction at a number of increasing field strengths, each field being reversed several times before readings were taken.

In order to make allowance for the slow time-change in the properties of the wire, the determinations of magnetisation and effect of change of tension on magnetisation were then repeated, the nickel wire being rewound with the same number of turns of insulated copper wire.

Finally, the contraction of the wire was once more determined at the temperature 10° C.

Values of the expression (1) were calculated from both sets of magnetisation observations, and the mean of the two sets compared with the actual contraction observed between them.

The results are shown in Tables II, III, IV, V, VI, in which values of all the quantities (obtained from the corresponding curves) are given for the same set of field strengths.

In Tables II, IV, the first column contains values of the field strength H , the second the magnetisation I at load 4.9 kg., the third the change of magnetisation δI accompanying an increase of load of 7 kg., the fourth the corresponding values of expression (1), i.e., the calculated value of $\delta l/l$. The numbers in the second, third, and fourth columns were determined in November, 1897. The fifth, sixth, and seventh columns contain values of I , δI , and $\delta l/l$, determined in January, 1898.

Tables III and V contain values of the field, the mean values of I and $\delta l/l$ calculated from Tables II and IV, the actual change of length α in millionths, observed about the end of December, 1897, and the difference $(\alpha - \bar{\delta l/l})$ between the observed and mean calculated elongations.

Finally, in Table VI are given the values of I and $\delta l/l$, measured in January; the mean α of the actual changes of length observed in December and in January, the latter being determined after the second set of magnetisation measurements; and the difference, $(\bar{\alpha} - \delta l/l)$, representing the corrected elongation at the time when the second set of measurements of I and $\delta I/\delta P$ was made.

Tables II and IV show the nature of the slow time-change in the magnetic behaviour of the nickel wire, the magnetisation at any field and load being less in January than in November.

The effect of this change on $\delta I/\delta P$ is very marked at low fields, though slight at higher fields; $\delta I/\delta P$ appears to diminish rapidly at low fields, as time goes on. This is remarkable, because the magnetic contraction at low fields seemed to change but slowly with time, and more rapidly at higher fields.

The change of length observed in January was nearly the same as in December (about four weeks earlier). At medium fields it was rather greater, but this may be due to a slight annealing caused by the repeated warming and cooling during the determinations of the *magnetisation* earlier in January.

A comparison of Tables II and IV, or III and V, shows again the nature of the influence of temperature on magnetisation and contraction, but as the measurements at the two temperatures were not made quite at the same time, the results probably do not accurately represent this influence.

If the results given here are compared with those given in the former paper, it will be seen that the magnetic contraction at any field observed in December, 1897 (Table III), is practically identical with the former value, but that the magnetisation curve is very different from the former one. The present magnetisation is much greater at low and medium fields, but about the same as before at higher fields. Further, the calculated value of the contraction is, especially at medium and high fields, considerably less than before. Hence the "corrected" contraction $-(\alpha - \epsilon l/l)$ cannot now be the same function of the magnetisation as before; it is, in fact, now much more nearly proportional to I^2 , as the last two columns of Tables III, V, and VI show.

It is impossible to say how much this discrepancy is due to the slow change which appears to be always going on in the magnetic properties of the nickel wire. In the former experiments the magnetisation was determined first, and a short time after the elongation was observed, but no allowance was made for any change of magnetisation which might have taken place in the meantime. Still, it is improbable that that would entirely account for the discrepancy.

Some of the above results are shown graphically in figs. 1, 2, 3. In fig. 1 the difference of ordinates of the highest and lowest curves for any value of the field H represents the effect on the magnetisation of changing the load from 1.4 kg. to 8.4 kg., after the additional load of 7 kg. has been applied and removed, and the field reversed, several times. The intermediate curve is the magnetisation curve of increasing reversals for the mean load 4.9 kg. These curves were determined in January, 1898.

In fig. 2 the curves represent, as functions of the field, the observed contraction (December, 1897), the calculated contraction (mean of November, 1897, and January, 1898), and the corrected contraction, i.e., the difference between the observed and calculated contractions. The calculated contractions of November and January are represented by the points $+++..$ and $\odot\odot\odot..$ respectively. The observed contraction curve is practically the same as that given in the former paper, but the calculated and corrected curves show considerable differences.

In fig. 3 the points marked $+++..$ represent the corrected contraction as a function of I^2 (Table III), and these points lie nearly on a straight line through the origin. The temperature during all the experiments represented by these curves was 10°C .

FIG. 1.

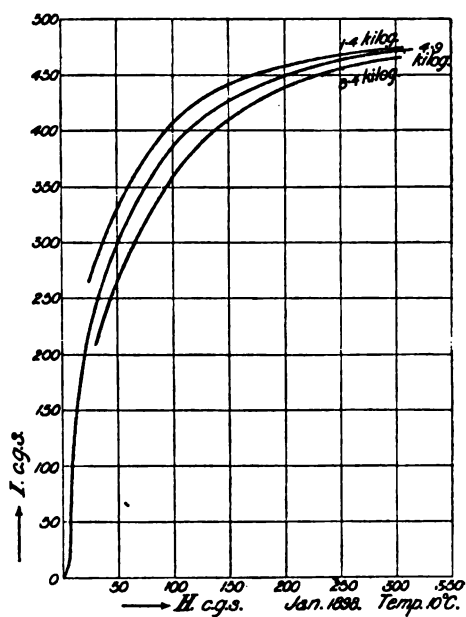


FIG. 2.

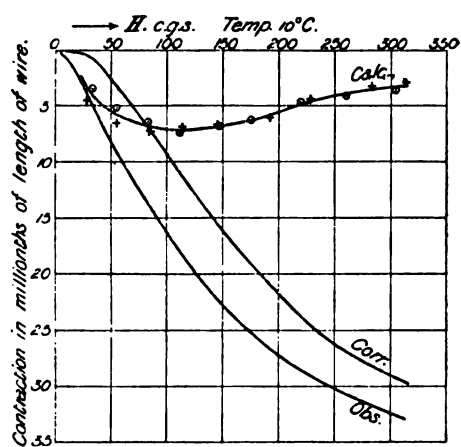


FIG. 3.

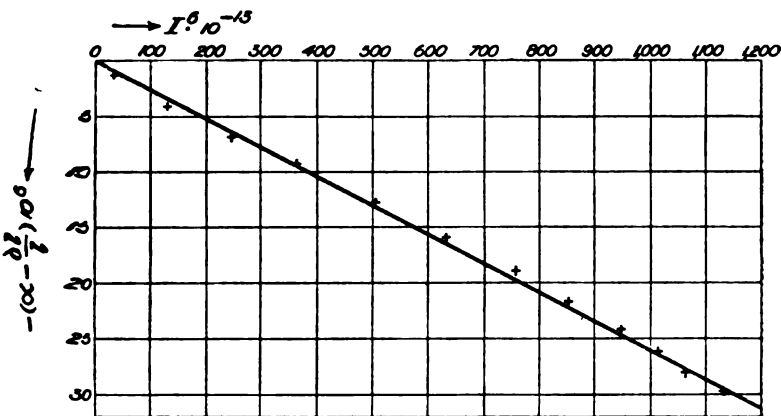


Table I.

Load 4.9 kg. August, 1897.

H.C.G.S.	Contraction of nickel wire in millionths of its length.	
	At 19° C.	At 56° C.
35	7.0	7.5
60	12.5	13.2
80	16.5	16.7
100	20.0	19.6
125	23.6	22.8
150	26.5	25.3
175	29.0	27.6
200	31.4	29.8
225	33.6	31.6
250	35.5	33.3
275	37.0	34.6
300	38.2	35.7
330	39.1	36.8

Table II.

Temperature 10° C. Load = 4.9 kg. $\delta P \times$ Section of Wire = 7 kg.

H.C.G.S.	November, 1897.			January, 1898.		
	I.	$\delta I.$	$\frac{\delta l}{l} \cdot 10^6$ calc.	I.	$\delta I.$	$\frac{\delta l}{l} \cdot 10^6$ calc.
35	272	-94.5	-5.1	258	-70.0	-3.7
60	340	-73.5	-6.6	323	-60.0	-5.5
80	375	-58.0	-7.1	359	-54.0	-6.45
100	399	-47.0	-7.15	385	-47.5	-7.05
125	419	-38.0	-7.0	409	-38.0	-7.1
150	434	-30.5	-6.7	426	-31.0	-6.7
175	446	-24.0	-6.3	440	-24.0	-6.3
200	454	-19.0	-5.65	450	-18.5	-5.5
225	461	-14.5	-4.6	458	-14.0	-4.65
250	466	-11.0	-4.0	464	-11.5	-4.2
275	470	-9.0	-3.55	468	-9.5	-3.8
310	476	-8.0	-3.15	473	-8.0	-3.5

Table III.

Temperature 10° C. Load = 4.9 kg.

H.	Mean I.	Mean $\frac{\delta l}{l} \cdot 10^6$ calculated.	$\alpha \cdot 10^6$ observed.	$(\alpha - \frac{\delta l}{l}) \cdot 10^6$	$I^6 \cdot 10^{-13}$.
35	265	-4.40	-5.3	-0.9	34.6
60	331	-6.05	-10.0	-3.95	132.0
80	367	-6.78	-13.35	-6.6	244.0
100	392	-7.10	-16.35	-9.2	363.0
125	414	-7.05	-19.9	-12.8	503.0
150	430	-6.70	-22.8	-16.1	632.0
175	443	-6.30	-25.3	-19.0	756.0
200	452	-5.58	-27.35	-21.8	852.0
225	460	-4.62	-28.9	-24.3	948.0
250	465	-4.10	-30.3	-26.2	1012.0
275	469	-3.70	-31.5	-27.8	1064.0
310	474	-3.30	-32.9	-29.6	1134.0

Table IV.

Temperature 55° C. Load = 4.9 kg. $\delta P \times$ Section of Wire = 7 kg.

H.	November, 1897.			January, 1898.		
	I.	$\delta I.$	$\frac{\delta l}{l} \cdot 10^6$ calc.	I.	$\delta I.$	$\frac{\delta l}{l} \cdot 10^6$ calc.
35	274	-93.0	-4.8	264	-70.0	-3.65
60	340	-67.0	-6.15	326	-62.5	-5.45
80	372	-55.5	-6.75	359	-55.0	-6.4
100	398	-46.5	-6.8	385	-44.8	-6.76
125	418	-35.0	-6.5	409	-35.5	-6.6
150	427	-27.5	-6.1	425	-27.0	-6.0
175	438	-22.0	-5.55	436	-21.0	-5.45
200	446	-18.0	-5.0	446	-16.8	-4.9
225	453	-14.2	-4.55	453	-12.9	-4.2
250	457	-11.5	-4.1	457	-10.5	-3.65
275	461	-8.8	-3.65	460	-7.5	-3.60

Table V.

Temperature 55° C. Load = 4.9 kg.

H.	Mean I.	Mean $\frac{\delta l}{l} \cdot 10^6$ calc.	$\alpha \cdot 10^6$ obs.	$(\alpha - \frac{\bar{\delta l}}{l}) 10^6$.	$I^2 \cdot 10^{-12}$.
35	269	-4.2	-5.5	-1.3	37.9
60	333	-5.8	-10.0	-4.2	136.0
80	365	-6.6	-13.8	-6.7	236.0
100	389	-6.8	-16.3	-9.5	347.0
125	411	-6.5	-19.4	-12.9	482.0
150	426	-6.0	-22.2	-16.2	597.0
175	437	-5.5	-24.5	-19.0	697.0
200	446	-4.9	-26.3	-21.4	787.0
225	453	-4.4	-27.8	-23.4	864.0
250	457	-3.9	-29.1	-25.2	911.0
275	460	-3.6	-30.2	-26.6	948.0

Table VI.

H.	January, 1898.		Mean $\alpha \cdot 10^6$ obs.	$(\bar{\alpha} - \frac{\delta l}{l})10^6$.	$l^3 \cdot 10^{-12}$.
	I.	$\frac{\delta l}{l} \cdot 10^6$ calc.			
35	258	-3.7	-5.30	-1.6	29.5
60	323	-5.5	-10.1	-4.6	114.0
80	359	-6.45	-13.6	-7.2	214.0
100	385	-7.05	-16.7	-9.6	326.0
125	409	-7.1	-20.2	-13.1	468.0
150	426	-6.7	-23.1	-16.4	598.0
175	440	-6.3	-25.5	-19.2	726.0
200	450	-5.5	-27.5	-22.0	830.0
225	458	-4.65	-29.05	-24.4	923.0
250	463	-4.2	-30.4	-26.2	986.0
275	468	-3.8	-31.6	-27.8	1050.0
310	473	-3.5	-32.9	-29.4	1120.0

“Upon the Structure and Development of the Enamel of Elasmobranch Fishes.” By CHARLES S. TOMES, M.A., F.R.S. Received February 7,—Read February 17, 1898.

(Abstract.)

The nature of the hard polished outer layer of the teeth of this group of fishes has been from time to time a subject of discussion, some authors holding that it is enamel, whilst others deny its claim to be so styled.

The author describes its physical, chemical, and histological peculiarities, calling attention to its hardness, its optical properties, its almost entire solubility in weak acids, and to its tubularity, in all of which respects it resembles unquestionably an enamel.

But it contains lacunar spaces, and presents a very distinct lamination, parallel, or nearly so, with its surface, in which respect it is unlike an enamel.

Still upon the balance of its characters it has much more in common with enamel than with dentine, from which it is sharply marked off by the entire absence of any collagen basis.

It is also shown that the tubular structure, which may be regarded as typical in these fish, passes by insensible gradations into a simple tissue differing but little from an ordinary enamel; this is especially the case where the whole layer is thin, as in the Rays. But the study of its development raises the difficulty afresh.

Each dentine papilla forms upon its surface a specialised layer which is derived from spindle-shaped cells, sending out immensely elongated processes which run nearly parallel with the surface. There is a large amount of intercellular substance formed so that the cell processes ultimately become inconspicuous, and a lamination or fibrillation of this layer only remains; in this stage it is exceedingly resistant to staining.

The layer is also permeated by cell processes which run through it at right angles to its surface, and these persist. It is found that this layer is the site of the so-called enamel formation, the first named cell processes giving rise to its lamination, and the last mentioned cell processes giving rise to the tube system which permeates it.

In all mammals dentine calcification commences at the very outside of the dentine papilla, and nothing at all corresponding to this specialised layer exists at any period.

But in the Elasmobranch fishes the calcification of the dentine, whether it be an osteodentine as in *Lamna* or a fine-tubed dentine as in most others, does not take place at the outer surface of the entire dentinal papilla, but along the deeper side of the specialised layer, thus soon cutting it off from any free communication with the body of the pulp.

This layer, in the extent to which it is developed, bears a ratio to the thickness of the ultimate "enamel." Over it lie the columnar epithelial cells of the enamel organ, which present several peculiarities; they are found to grow to three or four times their original size quite suddenly; that is to say, on one tooth germ they are small, on the very next they are enormous, and this is the tooth germ in which the specialised layer has attained to its maximum. Then on the next older germ the ameloblasts have fallen to their original size, and have lost their distinctness of outline.

The great growth of these cells just at one particular stage of tooth development, their subsequent immediate atrophy, and the fact that their length bears also a direct ratio to the thickness of the enamel formed, renders it impossible to suppose that they can be functionless, and it is suggested that they furnish the lime salts for the calcification of the specialised layer of the dentine papilla before alluded to, there being difficulties in the way of supposing that the dentine papilla does so.

In that case the disputed enamel layer of the finished tooth does not correspond precisely either to the enamel or to the dentine of the completed mammalian tooth, but is a joint product of the epiblastic enamel organ and the mesoblastic dentine papilla.

The general conclusion arrived at is that, just as the whole teeth of the Elasmobranchs present the simplest known form of tooth

development, so do they also present the first introduction of enamel as a separate tissue.

In its first introduction it was a joint product, made under circumstances which almost precluded any slow and gradual formation of an outer layer upon the teeth; but in the further specialisation of teeth in reptiles and mammals the tooth germs sink more deeply into the submucous tissue, and are protected for a much longer time.

The enamel organs become more specialised, and finally take upon themselves the entire work of enamel building, manufacturing both the organic matrix and furnishing it with lime salts, as unquestionably happens in mammals.

And if these conclusions be correct, it would be quite justifiable to call it enamel, even though the dentine papilla has had a share in its production.

“On Apogamy and the Development of Sporangia upon Fern Prothalli.” By WILLIAM H. LANG, M.B., B.Sc., Lecturer in Botany, Queen Margaret College, and “G. A. Clark” Scholar, Glasgow University. Communicated by Professor F. O. BOWER, Sc.D., F.R.S. Received February 28,—Read March 3, 1898.

(Abstract.)

The two most important deviations from the normal life-history of ferns, apogamy and apospory, are of interest in themselves, but acquire a more general importance from the possibility that their study may throw light on the nature of alternation of generations in archegoniate plants. They have been considered from this point of view by Pringsheim, and by those who, following him, regard the two generations as homologous with one another in the sense that the sporophyte arose by the gradual modification of individuals originally resembling the sexual plant. Celakovsky and Bower, on the other hand, maintain the view that the sporophyte, as an interpolated stage in the life-history arising by elaboration of the zygote, is not the homologue of the gametophyte, and is only represented in a few thallophytes. In the light of the theory of antithetic alternation no weight is attached to apogamy and apospory for phylogenetic purposes.

In the paper of which this is an abstract the results obtained by cultivating the prothalli of a number of species of ferns under conditions slightly different from the natural ones are described, and their bearing on the problem of the nature of alternation considered. *The behaviour of Scolopendrium vulgare*, Sm., and *Nephrodium dilatatum*.

tum, Desv., in which sporangia were borne upon the prothallus, has already been described in a preliminary statement.* It is therefore sufficient to express the results of prolonged cultivation of these and the remaining species in a tabular form.

Table of the Results of cultivating Prothalli for a Period of Two Years and a Half.

[Note.—In every species normal embryos were produced when conditions permitted fertilisation.]

Name.	Result.
<i>Scolopendrium vulgare</i> , Sm., var. <i>ramulosissimum</i> .	Gametophytic budding. Development of archegonial projections. Development of cylindrical process usually from the apical region of the prothallus. Apogamy. { Tracheides in cylindrical process. Leaves, roots, and ramenta on process. Sporangia on the process. Vegetative buds from tip of cylindrical process, or in place of an archegonial projection.
<i>var. marginale</i> .	Similar to var. <i>ramulosissimum</i> , but no sporangia, isolated ramenta, or leaves found.
<i>Nephrodium dilatatum</i> , Desv., var. <i>cristatum gracile</i> .	Gametophytic budding. Development of archegonial projections. Development of cylindrical process, usually from the under surface just behind the apex, which formed a "middle lobe." Apogamy. { Tracheides in middle lobe and cylindrical process. Sporangia, sometimes associated with ramenta, on middle lobe and process. No vegetative buds.
<i>Nephrodium Oreopteris</i> , Desv., var. <i>coronans</i> .	Gametophytic budding. Development of archegonial projections. Development of cylindrical process from apex of prothallus. Apogamy. { Tracheides in cylindrical process. Ramenta on cylindrical process. Vegetative buds (rare).

* 'Roy. Soc. Proc.,' vol. 60, p. 250.

<i>Name.</i>	<i>Result.</i>
<i>Aspidium aculeatum</i> , Sw., var. <i>multifidum</i> .	Gametophytic budding. Development of archegonial projections. Apogamy. { Tracheides in prothallus. Vegetative buds (rare).
<i>Aspidium angulare</i> , Willd., var. <i>foliosum multifidum</i> .	Gametophytic budding. Development of archegonial projections. Apogamy. { Ramenta on prothallus. Vegetative buds (frequent).
var. <i>acutifolium multifidum</i> .	Gametophytic budding. Development of archegonial projections. No apogamy seen.
<i>Athyrium niponicum</i> , Mett., normal form.	Gametophytic budding. Development of archegonial projections. Apogamy. { Tracheides in prothalloid growths from archegonial projections.
var. <i>cristatum</i> .	Similar to the normal form, but in addition a few apogamously produced vegetative buds.
<i>Athyrium Filix-femina</i> , Bernh.	Gametophytic budding.
var. <i>pericristatum</i> .	Development of archegonial projections.
var. <i>cruciatum cristatum</i> .	Development of cylindrical process from apex or from under surface of the prothallus.
var. <i>coronatum</i> .	Apogamy. { Tracheides in process. Continuation of process as a leaf. Vegetative buds.
<i>Polypodium vulgare</i> , L., var. <i>grandiceps</i> .	Gametophytic budding. Apogamy. { Isolated leaf-like growths. Vegetative buds (numerous).
<i>Aspidium frondosum</i> , Lowe (from the Pits, Royal Gardens, Kew).	Apogamy. Vegetative buds produced on short cylindrical processes before the culture had been watered. After the culture was watered, normal embryos.

In addition to the species mentioned in the table above, cultures were made of crested and uncrested forms of *Nephrodium Filix-mas*, Rich., representing the three sub-species, which are sometimes distinguished in this country. Some of these (both crested and normal) behaved in a similar manner to the species referred to in the table, though only one instance of apogamy induced by long cultivation has as yet been found. Others (crested and normal forms) produced a single bud on the under side of the prothallus which did not bear archegonia.

Connecting this latter type of apogamy, which agrees with the description of De Bary and Kny, with the more normal prothalli, was one variety, the archegonia of which developed into typical arche-

gonial projections. In the place of the projection nearest to the apex a vegetative bud arose.

It is possible to draw some general conclusions from this series of cultures. It is a striking fact that in every one of the species, prothalli, which under normal conditions would have produced normal embryos, became, after a longer or shorter period, apogamous. Further there was a general similarity in the changes of form and structure of the prothallus, which preceded this result. This form of apogamy, occurring after prolonged cultivation of normal prothalli under special conditions, may be distinguished as *induced apogamy*, in contradistinction to *direct apogamy*, by which is meant the immediate production of vegetative buds by prothalli, which are usually incapable of being fertilised. Both forms occur in *Nephrodium Filix-mas*.

The causes which appeared to induce apogamy in these prothalli were the prevention of contact with fluid water which rendered fertilisation impossible, and the exposure to direct sunlight. Possibly the temperature also had some effect. The case of *Nephrodium Filix-mas* shows that the variable condition of the sporophyte, as indicated by cresting, &c., though possibly predisposing to the changes which lead to apogamy, does not stand in any necessary connection with the phenomenon.

That different degrees of apogamy are distinguishable was also shown by these cultures. The cylindrical process, arising from the apex of the prothallus, or from its under surface, is to be regarded simply as a modification in form and structure of the gametophyte dependent on the altered conditions, and possibly a direct adaptation to these. The next stage is seen in cylindrical processes, which, while bearing sexual organs, also produce isolated members of a sporophyte (roots, rammenta, sporangia). It is to be borne in mind, however, that tissue differing from the rest of the process always occurred beneath the last-named structures. The final stage is the production of a vegetative bud capable of further growth as a typical sporophyte. In this a series, leading from the bud arising by transformation of the tip of a cylindrical process, to buds produced on or in the place of archegonial projections, and from this to buds situated on the under surface of the prothallus itself, can be recognised.

The readiness with which the intermediate form between gametophyte and sporophyte and the early stages of vegetative buds reassume the prothalloid form, is worthy of note, as bearing on some cases of apospory.

These departures from the normal development of the prothallus are not regarded as reversions in the ordinary sense, but as indications of the capability of direct response to altered conditions

possessed by the gametophyte. Their possible importance in relation to the theory of homologous alternation appears to the writer to be of this nature. If that theory be true, the sporophyte and gametophyte are modifications of a similar form. The gametophyte, especially the simple free-living prothallus of the Ferns, has departed less widely from that form. Such an organism as a fern prothallus would therefore appear to be suitable for experimental work, in the hope that its behaviour under altered conditions would afford hints as to the sort of changes which, in the original algal form, led to the evolution of the sporophyte. The altered conditions in this series of experiments are of a similar kind to those which are assumed by Professor Bower to have occurred on the spread of algal forms to the land, and to have conduced to antithetic alternation.

The results may now be used in picturing the manner in which alternation of generations might have come about by the modification of originally similar individuals into gametophyte and sporophyte. It is assumed for this purpose that the sporophyte of the vascular cryptogams did not arise by the elaboration of a structure resembling a bryophytic sporogonium. It is recognised that the theory of antithetic alternation, as elaborated by Professor Bower, affords a consistent and satisfactory explanation, if the assumptions necessitated by the theory are granted. The present theory, which is put forward merely as a provisional hypothesis, is founded on another class of facts.

With the spread of algal organisms to the land, where in the absence of any vegetation affording shade, some at least would be exposed to more intense illumination, the flattened form would probably be assumed. Prolonged drought and the influence of direct sunlight, inducing directly a change of form into a cylindrical body, might be accompanied by the substitution of a reproductive organ forming dry reproductive cells (spores) for those adapted to an aquatic existence. The acquisition of more highly developed absorbent organs (primitive roots) would further the existence and growth of this modified gametophyte. This spore-producing stage would at first follow the sexual stage in any individual exposed to dry conditions. It is possible to imagine, however, how the association of the asexual with the sexual individual might come about. Absence of fluid water would prevent the liberation of motile spores from the zygote. The latter would be obliged to germinate *in situ*, and the fact that it did so under dry conditions would tend to the shortening of the sexual stage, and the speedy assumption of the sporophytic form and mode of reproduction. From the spore, which would always separate from the parent, a sexual individual would *arise, since germination* could only take place in a damp spot. As

soon as, with the increase in size and complexity of the spore-bearing plant, a vegetation capable of affording shade came into existence, the conditions suitable for the persistence of the more primitive, alga-like, sexual stage in the life history would be present. The latter has, of course, also been modified in various ways.

In the concluding portion of this paper, the theories of antithetic and homologous alternation are compared by considering the explanations they afford of the facts. The general conclusion reached is that, while both afford a *possible* explanation of the facts of alternation in archegoniate plants, any evidence which would render one or the other untenable is wanting. The reasons on which either is considered more probable depend on the views held as to the lines of descent which have been followed, and the degree to which the different groups of archegoniate plants have had a common origin, or represent actual steps in the process of evolution of the sporophyte. Under these circumstances the question must be regarded as an open one until the available lines of evidence have been more fully investigated.

I am especially indebted to Dr. Scott and Professor Bower for their assistance and advice; the work was commenced in the Jodrell Laboratory of the Royal Gardens, Kew, and subsequently carried on in the Botanical Laboratory of the University of Glasgow.

“Experimental Observations on the early Degenerative Changes in the Sensory End Organs of Muscles.” By F. E. BATTEN, M.D. Communicated by Professor VICTOR HORSLEY, F.R.S. Received February 17,—Read March 3, 1898.

(Abstract.)

The experiments described in the following paper were undertaken in order to show, firstly, that degeneration occurred in the first place in that part of the neuron most remote from the cell, and secondly, to reproduce within the muscle-spindle, if possible, certain changes which had been shown by the author to be present in the case of *tabes dorsalis* in man.

The method of experiment was as follows:—Dogs were selected, and the mixed roots of the 5th cervical to the 1st dorsal inclusive were divided, and the animals killed at the following periods after section of the nerve, viz., 24, 48, 72, 96, 120 hours, and 7 and 14 days.

From the biceps muscle after being treated by Sihler's method muscle-spindles were teased out; some of these were mounted without further staining, others were treated by Marchi's method, others were stained by the *Marchi-Pal* method.

62 *Degenerative Changes in Sensory End Organs of Muscles.*

The normal muscle-spindle showed the existence of a spiral form of nerve termination in connection with the large nerve fibre that passes to the equatorial region of the spindle, this spiral nerve termination is shown to wind round a muscle fibre, which at this point contains large cells, at one point completely filling the muscle fibre and interrupting the striation, but tailing off in either direction so that the cells come to lie in the centre of the muscle fibre. It is then shown that in twenty-four hours after section of the nerve changes may be seen in this spiral termination, and that in forty-eight hours after section of the nerve the spiral is no longer recognisable, oval and elongated granular cells now making their appearance. Changes then appear in the large intramuscular cells. The musculo-spiral nerve was then examined in three parts of its course, (1) in the muscle, (2) at its entrance into the muscle, (3) near its origin, at various periods after section of the nerve. No obvious change could be found in the nerve till between the fifth and seventh day after section of the nerve, and at that time degeneration was as marked in the central portion of the nerve as in the peripheral (Marchi and Marchi-Pal methods were used).

The existence of a spiral form of nerve termination has already been described by Ruffini as encircling a muscle fibre, and other authors refer to a spiral within the muscle-spindle; but it has not, I believe, been shown that the spiral encircles the large intramuscular cells first described by Kühne.

Early degeneration was first described by Cattaneo in the nerve termination in the musculo-tendon organ twenty hours after section of the nerve. Both these investigators used the gold-chloride method, in the present research Sihler's method has been used.

The results of the research have been to show—

(1) That within the muscle-spindle a spiral form of nerve termination exists surrounding a fine muscular fibre, in the centre of which are large, clear, non-nucleated cells.

(2) That changes take place in the spiral in twenty-four hours after section of the nerve, and that such changes become marked in forty-eight hours.

(3) That degeneration of the medullated sheath of the nerve takes place in the whole course of the nerve at the same time after section of the nerve.

(4) That no fatty change could be demonstrated in the intramuscular cells by the Marchi method similar to those found in the case of *tabes dorsalis* in man.

March 17, 1898.

THE LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Professor Wilhelm Pfeffer, who was elected a Foreign Member in 1897, was admitted into the Society.

THE CROONIAN LECTURE, "The Nature and Significance of Functional Metabolism in the Plant" (*Das Wesen und die Bedeutung des Betriebsstoffwechsels in der Pflanze*), was delivered by Professor W. Pfeffer, For. Mem. R.S., of the University of Leipzig.

The following Papers were read :—

"On the Intimate Structure of Crystals. Part III. Crystals of the Cubic System with Cubic Cleavage. Part IV. Cubic Crystals with Octahedral Cleavage." By **PROFESSOR SOLLAS**, F.R.S.

March 24, 1898.

SIR JOHN EVANS, K.C.B., D.C.L., Treasurer and Vice-President, in the Chair.

The Right Hon. Sir Herbert Eustace Maxwell, a Member of Her Majesty's Most Honourable Privy Council, was admitted into the Society.

THE BAKERIAN LECTURE, "Further Experiments on the Action exerted by certain Metals and other Bodies on a Photographic Plate," was delivered by **DR. W. J. RUSSELL**, V.P.R.S.

The following Paper was read :—

"A Photographic Investigation of the Absorption Spectra of Chlorophyll and its Derivatives in the Violet and Ultra-violet Region of the Spectrum." By **C. A. SCHUNCK**. Communicated by **DR. E. SCHUNCK**, F.R.S.

"On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force and the Period of Solar Spot Frequency." By WILLIAM ELLIS, F.R.S., formerly of the Royal Observatory, Greenwich. Received March 3, —Read March 10, 1898.

(Second Paper.)

In a paper communicated to the Royal Society in the year 1879, and printed in the 'Philosophical Transactions' for 1880, I compared the diurnal range of magnetic declination and horizontal force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, with the corresponding numbers of sun-spot frequency as determined by the late Dr. Rudolf Wolf, of Zürich. As I then said, I conceived that the long series of Greenwich observations, made throughout on the same general plan and with instruments of the same kind, might be applied as a valuable independent test of the reality of the relation generally understood to exist between the phenomena in question. And the comparison appeared to be distinctly confirmatory thereof. For it was to be observed that, although the sun-spot period, commonly called the eleven-year period, varied in length to the extent of several years, the corresponding magnetic periods varied in a similar manner. Still, in a case of this kind, in which the cause of the phenomena observed has not been determined or ascertained, it becomes important indisputably to prove the accuracy of the observed facts and of the inference to which they lead. And if further observation shows that the phenomena continue to progress collaterally, the circumstance must eventually be accepted as indicating that between the two phenomena there exists a more or less direct relation which, in any theoretical consideration of the subject, could not be ignored. The previous paper, as mentioned, includes results to the year 1877; but the material since accumulated, available now to 1896, happens to be especially interesting, and, contrasting in some respects with the earlier portion, is worthy of being made known, the series as a whole forming one continuous chain of evidence that much strengthens the argument for relation. Apart, however, from any individual opinion on the matter, it is well that, so long as the phenomena observed remain without explanation, the facts thereof should be carefully set forth. I propose, therefore, to discuss the question anew for the whole period 1841 to 1896.

It is unnecessary to say anything in explanation of the results given in the previous paper. I will therefore proceed to describe *the new work*, extending from 1878 to 1896. As before, the mean

diurnal range of declination in each individual month is taken to represent (relatively to other months) the magnetic energy of the month, and similarly for horizontal force. By the mean diurnal range is to be understood a number formed as follows. Means of the indications at each separate hour of the day being taken through a month (omitting days of extreme disturbance) the difference between the least and the greatest of the twenty-four mean values is the monthly mean diurnal range. The numbers are obtained from the Greenwich annual volumes, but those for the years 1895 and 1896 not having been yet published, the Astronomer Royal has kindly allowed me to use them as necessary for the purposes of this paper. The very small correction for temperature required by the horizontal force results from 1878 to 1882 as printed, has been duly applied: beginning with 1883, the values are printed corrected for temperature.

Thus is obtained, both for declination and horizontal force, results strictly comparable with those of the previous paper, giving in all a series of results for fifty-six years. In any graphical representation of unexplained phenomena, it is most important that there should be ready reference to the numerical data on which it is founded, to enable those who might wish to test the work the means of so doing without great inconvenience, otherwise the graphical representation alone can carry no proper conviction. The numbers for the years 1841 to 1877 are to be found in the previous paper; it is therefore necessary to give here only the corresponding numbers from 1878 to 1896. These are contained in Table I. The numbers for horizontal force are given, as in the previous paper, in parts of the whole horizontal force taken as unity. The relation in magnitude of the westerly force (declination) to the northerly force (horizontal force) will be understood by considering that one minute of arc of declination corresponds to 0.00029, that is 29 of the horizontal force unit of Table I. Examining now this table, it will be seen that there is an annual inequality in the magnitude of the diurnal range, the summer numbers being much greater than the winter numbers. In order, therefore, to estimate progressive change, it is convenient to form a number for each month that shall be free of annual inequality, to allow the progressive change better to appear. Assuming the different months to be of the same length this is done, as before, by taking the mean of each twelve consecutive monthly numbers, beginning successively with each individual month, first, say, with January, next with February, and so on, taking afterwards the mean of each two consecutive numbers so found, thus producing annual values free of annual inequality, which may be presumed to apply to the middle of each successive month. *The process is equivalent, suppose for the number for*

January, to adding together half the sum of the numbers for the preceding and following July, and the sum of the numbers for the intervening eleven months, August to June, and dividing the whole by twelve. These new monthly numbers, each expressing an annual mean, are given in Table II, both for declination and horizontal force, and they are used with those of Table II of the previous paper to form the two lower curves of the accompanying plate. I remarked in my previous paper that the indications of vertical force were for the present purpose not very manageable; several different instruments had been employed, and the results presented anomalies. Certain beneficial alterations were, however, made in 1882, in the instrument still in use, since which time it has worked better. It showed a maximum diurnal range in 1883, the descent to a minimum in 1889, and the subsequent rise to a maximum, although there remains still some degree of irregularity of action.

As regards sun-spot frequency, Dr. Wolf's monthly values, as derived directly from observation, are given for the years 1841 to 1877, in Table VI of my previous paper. Those for the years following 1878 to 1896 are to be found in different numbers of his '*Astronomische Mittheilungen*,' the values in the later years, after the death of Dr. Wolf, in 1893, having been similarly prepared by Professor Wolfer, his successor at Zürich. I am not aware that these have been before given in a collected form; they will be found in the annexed Table III. For the purpose of smoothing the accidental irregularities of these observed sun-spot numbers, Dr. Wolf treated them in the same way as the numbers of our Table I (expressing magnetic range) were dealt with to form those of Table II. Though the process was here employed for a reason different to that which rendered its application necessary in the case of magnetic range, the similarity of treatment happily makes the resulting numbers strictly comparable with the magnetic numbers. The smoothed sun-spot numbers, from 1841 to 1876, June, are to be found in a table contained in Dr. Wolf's paper, "*Mémoire sur la Période commune à la Fréquence des Taches Solaires et à la Variation de la Déclinaison Magnétique*."* Those from 1876, July, to 1896, added in our Table III, have been in part taken from the '*Astronomische Mittheilungen*' and in part calculated from the observed numbers contained in the same table. These smoothed values, with those of the preceding series taken from the paper above mentioned, are used to form the upper curve in the diagram of collected sun-spot and magnetic curves. It may be asked why the Greenwich magnetic variations are not compared with the Greenwich sun-spot record. But this record having been maintained only for some twenty years, it was deemed better to adhere throughout to the long Wolf series

* '*Memoirs of the Royal Astronomical Society*,' vol. 48, p. 199.

rather than endeavour to make reduction from one to the other for a portion of the series.

Examination of the collected curves will, I think, show that the extension of the period previously employed by inclusion of the new results, extending from 1878 to 1896, has produced curves that offer striking points of interest. Selecting the extreme points of the several curves, or, which is better, taking the successive least and greatest values from Table II, and from the corresponding table of the previous paper, for magnetic values, and from Table III, and from the corresponding table in vol. 43 of the 'Royal Astronomical Society Memoirs,' for smoothed sun-spot values, the following epochs of minimum and maximum are obtained :—

Table of Epochs of Magnetic and Sun-spot Minima and Maxima.

Reference No.	Phase.	Magnetic epochs.			Sun-spot epoch.	Excess above sun-spot epoch.		
		Declination.	Horizontal force.	Mean magnetic.		Declination.	Horizontal force.	Mean magnetic.
1	Minimum	1844·3	1842·9	1843·60	1843·5	+0·8	−0·6	+0·10
2	Maximum	1848·1	1849·0	1848·55	1848·1	0·0	+0·9	+0·45
3	Minimum	1857·2	1855·1	1856·15	1856·0	+1·2	−0·9	+0·15
4	Maximum	1860·6	1860·2	1860·40	1860·1	+0·5	+0·1	+0·30
5	Minimum	1867·5	1867·6	1867·55	1867·2	+0·3	+0·4	+0·35
6	Maximum	1870·8	1870·9	1870·85	1870·6	+0·2	+0·3	+0·25
7	Minimum	1879·0	1878·7	1878·85	1879·0	0·0	−0·3	−0·15
8	Maximum	1884·0	1883·8	1883·90	1884·0	0·0	−0·2	−0·10
9	Minimum	1889·5	1890·0	1889·75	1890·2	−0·7	−0·2	−0·45
10	Maximum	1893·5	1894·0	1893·75	1894·0	−0·5	0·0	−0·25
Mean excess (five epochs of minimum)						+0·32	−0·32	0·00
Mean excess (five epochs of maximum)						+0·04	+0·22	+0·13
General mean excess						+0·18	−0·05	+0·06

The mean magnetic epoch is taken to be the mean of those for declination and horizontal force. These vary somewhat for the epochs Nos. 1 and 3, but the mean epoch in both cases falls near to the sun-spot epoch.

Taking the differences between successive epochs of minimum and maximum 1—2, 2—3, &c., the following intervals are found :—

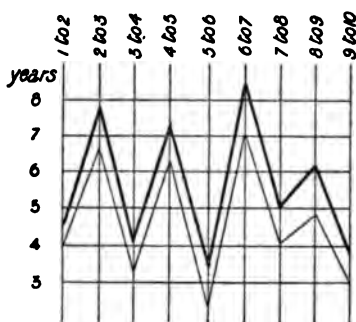
Intervals between successive Magnetic Epochs.

Min. to Max.	Max. to Min.	Min. to Max.	Max. to Min.	Min. to Max.	Max. to Min.	Min. to Max.	Max. to Min.	Min. to Max.
1—2.	2—3.	3—4.	4—5.	5—6.	6—7.	7—8.	8—9.	9—10.
4·95y	7·60y	4·25y	7·15y	3·30y	8·00y	5·05y	5·85y	4·00y

Intervals between successive Sun-spot Epochs.

4·60y	7·90y	4·10y	7·10y	3·40y	8·40y	5·00y	6·20y	3·80y
-------	-------	-------	-------	-------	-------	-------	-------	-------

FIG. 1.— Intervals between successive Sun-spot and Magnetic Epochs compared.



The thick line shows the variation in length of the interval between successive sun-spot epochs, and the thin line that between successive magnetic epochs. 1 to 2, 3 to 4, &c., indicate intervals from minimum to maximum, and 2 to 3, 4 to 5, &c., those from maximum to minimum.

These numbers are represented in a graphical form in fig. 1. They run so closely together that it became necessary, in the figure, to diminish the magnetic intervals by one hour, otherwise much of the magnetic curve would have fallen so near to the sun-spot curve as to become obliterated thereby. The general similarity of the variations in length of successive magnetic and sun-spot intervals, alternately from minimum epoch to maximum epoch, and from maximum epoch to minimum epoch, is thus very clearly seen. When the variation of the magnetic interval is large, that of the sun-spot interval is also large, and when the one is small, the other is also small.

The mean of the five intervals from minimum epoch to maximum epoch is for the magnetic effect 4·31y, and for the sun-spot effect 4·18y; the mean of the four intervals from maximum epoch to minimum epoch is for the magnetic effect 7·15y, and for the sun-spot effect 7·40y. Whole period for magnetic effect 11·46y, and for sun-spot effect 11·58y.

Taking further from the table of epochs, instead of successive intervals, the successive periods, 1—3, 2—4, &c., we have—

Length of Magnetic Period.

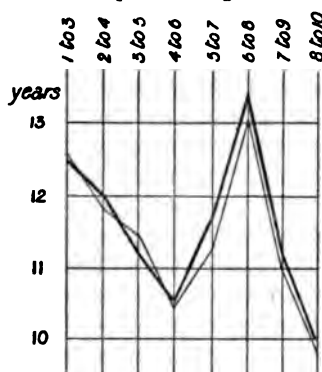
1—3.	2—4.	3—5.	4—6.	5—7.	6—8.	7—9.	8—10.
12·55y	11·85y	11·40y	10·45y	11·30y	13·05y	10·90y	9·85y

Length of Sun-spot Period.

12·50y	12·00y	11·20y	10·50y	11·80y	13·40y	11·20y	10·00y
--------	--------	--------	--------	--------	--------	--------	--------

The odd numbers, 1—3, 3—5, &c., indicate the periods between successive minimum epochs, and the even numbers those between successive maximum epochs. They are represented graphically in fig. 2. The similarity of the variation in length of the complete

FIG. 2.—Length of Sun-spot and Magnetic Periods compared.



The thick line shows the variation in length of successive sun-spot periods, and the thin line that between successive magnetic periods. Odd numbers indicate periods from minimum to minimum, and even numbers periods from maximum to maximum.

magnetic and sun-spot periods, amounting to above three years, is here well brought out. Mean magnetic period 11·42y. Mean sun-spot period 11·57y. These values differ slightly from those above given, owing to the numbers becoming here combined in a little different way. Fig. 2 suggests a suspicion that the period decreases in length through several periods, then increases for several periods, and so on. But what may be the order of such variations is a question to which at present it does not seem possible to give any reliable answer.

Examining further the collected curves, it is seen that the maximum points of the curves have at different epochs very different degrees of intensity. If for each curve we arrange the epochs of maximum in order, with reference thereto, we find as follows:—

Order of Epochs with reference to intensity of Sun-spot and
Magnetic Effects.

Sun-spot epoch.	Corre- sponding number.	Declination epoch.	Corre- sponding number.	Hor. force epoch.	Corre- sponding number.
1870	140·5	1870	12·76	1870	308
1848	131·5	1848	12·46	1860	295
1860	97·9	1860	11·42	1848	268
1894	87·9	1894	10·42	1894	233
1884	74·6	1884	9·84	1884	221

In each curve the greatest and least maxima are those of 1870 and 1884 respectively, and the order in each case is similar excepting for the epochs of 1848 and 1860, which in horizontal force are reversed in position, although otherwise falling in with the general order of the other curves. The horizontal force observations have to be corrected for the effect of temperature (a correction not required for declination), and in the years 1848 and 1860 the magnets were in the original upper magnet room, in which the diurnal range of temperature was considerable, rendering difficult the determination of the actual temperature of the magnet (a bar 2 feet in length), and this may possibly account for the apparent displacement of the 1848 and 1860 epochs of maximum in horizontal force. In 1864 an underground basement was specially constructed for the magnets, in which the variation of temperature is small, and in this apartment the magnets have since remained. As regards the minimum points of the curves, the sun-spots at the epochs of 1856, 1867, 1879, and 1890 practically disappear, but not so at that of 1843, at which epoch the most elevated of the magnetic minima, both in declination and horizontal force, occurs, being so far in harmony with what has been pointed out as to the behaviour at maximum.

Considering that the irregularities in the length of the sun-spot period so entirely synchronise with similar irregularities in the magnetic period, and also that the elevation or depression of the maximum points of the sun-spot curve is accompanied by similar elevations and depressions in the two magnetic curves, it would seem, in the face of such evidence, that the supposition that such agreement is probably only accidental coincidence can scarcely be maintained, and there would appear to be no escape from the conclusion that such close correspondence, both in period and activity, indicates a more or less direct relation between the two phenomena, or otherwise the existence of some common cause producing both. The sharp rise from minimum epoch to maximum epoch, and the more gradual fall from maximum epoch to minimum epoch, may be pointed out as a characteristic of all three curves. The similarity of

the little drop shown in all the curves in 1882 and 1883 is also striking.

Observation has been supposed to indicate that the magnetic effect follows the sun-spot effect, so that a retardation, or what has been called a "lagging," of the magnetic effect exists, but the evidence for this has never appeared to me to be quite satisfactory. The fewer comparisons of sun-spot and magnetic epochs given in my first paper seemed to give support to such supposition. But taking the more extended comparison contained in the preceding table of epochs, it is seen that the declination epoch is on the whole retarded by $0.18y$ (fraction of year), whilst that of horizontal force is accelerated by $0.05y$. Mean retardation = $0.06y$. Looking at the irregularities in the numbers on which these means are founded, it seems doubtful whether, without a yet more extended comparison, any real difference or definite lagging can be said to exist. Two things, however, may be noted. One is, that the differences in the early part of the table are inclined to positive, and in the later portion to negative values. What will happen when further results are included? Considering, however, the strength of the evidence for some form of relation that has been shown by what has preceded to exist, it seems in every way probable that the individual differences of epoch represent in great part accidental residuals. Rather, indeed, is it not likely that to some extent small differences or irregularities may be expected to appear, when it is remembered that we are comparing together solar and terrestrial phenomena, to us probably only incomplete manifestations of involved actions, about which little is known and of which the cause is obscure. Especially, too, when, as regards the terrestrial effect, the magnetic variations are deduced from the observations at a single station, Greenwich. Consider also the composite character of the sun-spot phenomena at about the time of minimum epoch. After a maximum, as the following minimum epoch is approached, sun-spots become in successive years fewer in number and smaller in magnitude, the regions north and south of the solar equator in which spots appear tending, as the spots become less numerous, more and more towards the equator, until at the minimum epoch the spots disappear. At the same time, spots of, as it were, a new cycle come into view in high latitudes, and become rapidly more numerous and important until another maximum is passed, after which the spots become again less numerous, the regions in which they appear approaching as before the solar equator, until at the following minimum again the spots disappear. Thus the curve at the minimum epoch seems to be produced by the junction of an expiring cycle with a new outburst becoming rapidly active.

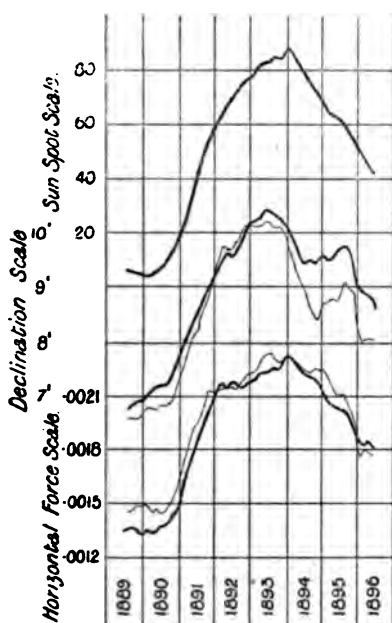
In tabulating the magnetic records at Greenwich, it has been the practice to include all days (except those of extreme disturbance),

consequently on many that are retained considerable disturbance exists. Now abnormal disturbance is more frequent, as well as greater in magnitude, towards the epochs of maximum sun-spot frequency, and tends almost to disappear at the epochs of minimum frequency. For instance, the sun-spot minima of 1856 and 1879 were both remarkable for little irregular disturbance, especially the latter, whilst at the sun-spot maximum of 1870, disturbance was unusually frequent and considerable in amount, the difference in this respect being very striking, as a mere inspection of the photographic records for the epochs mentioned would abundantly show. In preparing the former paper the question, therefore, did arise whether and to what extent the increased diurnal magnetic range, about the periods of sun-spot maximum, might be due to the greater prevalence at such periods of abnormal disturbance, but I then satisfied myself, by examination of the records, that this circumstance exercised no important influence on the results. Now, however, I can substantiate that conclusion by numerical data.

It is known to those acquainted with magnetic work that in order the better and more readily to compare together the diurnal magnetic inequalities for different places it has been the practice, since the year 1889, to tabulate the records at British observatories for five selected quiet days in each month, the selection of days being made by the Astronomer Royal. These quiet day results, unaffected by magnetic disturbance, show only the solar diurnal variation, and are now available for Greenwich, from 1889 to 1896. Mean hourly values being formed in each month from the indications on the five quiet days, the diurnal range of declination and horizontal force was found in the same way as before described for the full monthly values, the resulting numbers being given in Table IV. These were further treated for removal of the annual inequality, in the same way that the numbers of Table I were treated to form those of Table II. The values so found are contained in Table V, and are those represented graphically in fig. 3, adding thereto the graphical representation of the sun-spot and magnetic phenomena, for the corresponding years from the long series 1841—1896, in which all days (excepting those of extreme disturbance) were included.

The thick line in declination and horizontal force (fig. 3) shows the diurnal magnetic range as found from employing all days, including many of magnetic disturbance. The thin line, formed from five selected quiet days in each month indicates the true solar diurnal range. Two things thus appear. Firstly, that the solar diurnal range (thin line), in which the influence of irregular magnetic disturbance has no part, is itself really affected with a periodical variation similar to that of sun spots. And secondly, that the effect of *including disturbed days* (thick line) alters the solar diurnal range

FIG. 3.—Sun-spot Frequency and Diurnal Range of Declination and Horizontal Force compared.



The upper curve is that of sun-spot frequency. The middle and lower curves are those of diurnal range of declination and horizontal force respectively; the thick line is that resulting from the employment of all days, and the thin line that found from five selected quiet days in each month.

(thin line) in a small degree only as compared with its variation with sun spots, from which it follows that the diurnal range as found from employing all days (thick line), as in the 1841—1896 series, may be taken to represent the solar diurnal range (thin line) the variation of the former being essentially that of the solar diurnal range.

The explanation of the circumstance that the difference in the amplitude of the magnetic range, as found from five selected quiet days, and from all days, is so small as compared with the variation in amplitude of both with variation of sun spots, would seem to be that the effect of irregular magnetic disturbance is such as sometimes to increase the value of the magnetic element and sometimes to decrease it, these opposite effects combining to neutralise each other to such an extent as to influence the solar diurnal range in an unimportant degree only, as compared with its considerable variation with that of sun spots, which is the point that is here material.

Although the change produced in the character of the diurnal magnetic movement by including disturbed days is not a matter that possesses significance as regards the purposes of this paper, it becomes in other respects one for consideration. It is, however, a question related to others that would be better discussed in a separate communication.

Table I.—Monthly Mean Diurnal Range of Declination and Horizontal Force, as deduced from observations made at the Royal Observatory, Greenwich. (The declination is expressed in minutes of arc; for horizontal force the unit is 0·00001 of the whole horizontal force.)

Year.	Declination.												Horizontal Force.											
	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1878	3·6	5·0	7·2	9·3	7·3	9·7	8·7	9·3	8·2	5·2	3·9	3·3	70	61	109	159	124	163	112	151	171	165	72	53
1879	3·5	4·4	6·9	8·3	8·3	9·0	9·2	10·2	8·0	6·1	4·3	3·9	54	78	84	171	172	113	201	185	168	154	73	58
1880	4·0	5·3	8·0	10·5	9·1	9·9	9·6	9·6	10·1	8·8	5·9	4·9	56	78	125	196	216	205	237	252	227	195	122	68
1881	5·4	6·5	9·6	10·6	10·5	12·2	11·4	12·1	10·9	8·4	6·3	5·9	80	149	159	217	222	270	270	245	240	199	139	103
1882	5·9	6·4	9·7	12·1	12·0	9·9	9·1	10·5	10·5	8·3	5·9	5·0	84	125	165	207	323	304	253	260	249	175	128	86
1883	5·3	7·6	9·2	11·4	10·2	10·9	12·3	10·6	11·2	10·1	6·6	5·9	98	119	181	254	257	237	238	247	226	159	99	99
1884	6·3	8·1	11·4	13·2	11·3	12·4	10·8	10·4	10·9	9·4	6·6	5·9	138	191	224	271	304	259	259	213	243	197	156	107
1885	6·9	6·0	9·5	11·2	11·0	11·7	11·7	11·4	9·6	7·3	5·9	4·5	106	58	140	203	239	248	260	255	211	190	117	71
1886	7·4	6·5	9·4	10·4	10·6	9·9	9·7	9·9	8·0	7·7	6·6	5·2	112	105	159	220	243	263	253	215	200	158	82	79
1887	5·6	7·1	7·4	9·1	9·5	9·7	10·2	9·6	8·5	6·8	5·0	5·6	92	89	131	210	223	224	224	224	163	126	74	77
1888	5·2	5·6	7·6	8·2	8·8	9·3	9·2	9·1	7·2	6·8	5·4	4·3	85	64	130	181	196	220	231	218	187	150	82	64
1889	3·6	5·2	6·7	8·6	8·4	8·6	8·2	8·4	7·1	6·4	4·9	3·9	55	73	122	158	187	210	195	190	178	135	68	57
1890	4·8	5·4	7·5	9·2	8·1	8·6	8·9	9·2	7·9	6·9	5·7	4·2	71	75	120	166	158	197	201	189	182	142	89	71
1891	4·6	5·5	8·6	9·9	11·1	9·6	11·0	10·5	9·3	9·5	6·8	5·1	56	89	130	207	217	223	259	252	240	203	143	86
1892	5·9	8·0	10·1	11·1	11·4	11·7	12·2	12·2	11·2	9·5	6·0	5·1	122	148	207	278	276	290	342	322	174	237	150	98
1893	6·2	7·8	10·9	13·6	12·7	13·4	12·9	13·1	11·3	9·3	7·4	5·7	104	121	207	293	261	335	333	315	252	223	149	112
1894	5·7	8·2	10·2	12·9	12·0	11·3	12·0	11·8	9·4	7·5	6·5	5·5	112	132	203	285	338	356	297	308	229	200	126	108
1895	6·3	8·3	9·5	12·6	12·4	13·7	12·1	10·1	9·4	8·2	7·4	5·5	95	120	168	205	268	335	267	219	220	187	111	72
1896	7·5	8·3	9·6	11·3	9·7	9·6	10·3	10·2	9·3	7·0	5·3	5·4	95	120	168	205	268	223	242	261	244	152	93	53

Table II.—Annual Means of the Monthly Mean Diurnal Range of Declination and Horizontal Force as deduced from Observations made at the Royal Observatory, Greenwich. (The declination is expressed in minutes of arc; for horizontal force the unit is 0.0001 of the whole horizontal force. The number standing under January represents the annual mean for the year, of which the middle point is January 15, and similarly for other months.)

Year.	Declination.												Horizontal Force.											
	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1877	6.92	6.80	6.83	6.87	6.88	6.90	132	133	133	132	128	123
1878	6.80	6.84	6.84	6.82	6.77	6.77	6.79	6.76	6.72	6.67	6.65	6.65	120	117	117	116	115	115	114	114	114	114	116	118
1879	6.65	6.70	6.73	6.76	6.82	6.84	6.86	6.92	7.00	7.14	7.27	7.34	121	125	127	128	129	129	130	130	131	134	137	140
1880	7.30	7.38	7.45	7.05	7.83	7.93	8.03	8.14	8.26	8.33	8.39	8.55	143	147	152	157	162	164	166	170	174	176	178	180
1881	8.72	8.90	9.03	9.05	9.11	9.17	9.19	9.19	9.19	9.25	9.38	9.35	185	186	186	187	187	187	190	192	191	190	199	204
1882	9.17	9.02	8.93	8.91	8.80	8.84	8.77	8.80	8.83	8.78	8.68	8.64	205	205	206	205	204	203	202	202	203	203	200	197
1883	8.80	8.93	8.96	9.07	9.17	9.21	9.27	9.33	9.44	9.61	9.73	9.84	200	202	200	202	205	207	200	214	218	221	219	216
1884	8.84	8.94	9.00	9.08	9.17	9.21	9.21	9.31	9.45	9.58	9.79	9.15	211	207	207	206	205	205	204	197	188	182	180	183
1885	9.15	9.23	9.22	9.08	8.96	8.88	8.87	8.95	8.97	8.93	8.88	8.79	184	186	186	186	183	180	178	181	183	185	186	185
1886	8.63	8.40	8.36	8.31	8.35	8.41	8.37	8.32	8.26	8.12	8.02	7.97	183	180	178	176	173	172	171	169	167	166	165	162
1887	7.98	7.90	8.00	7.98	7.87	7.83	7.82	7.75	7.69	7.66	7.60	7.55	160	160	160	160	158	157	157	155	154	153	151	150
1888	7.49	7.43	7.35	7.30	7.22	7.28	7.16	7.08	7.02	7.00	7.00	6.95	150	151	150	151	152	152	149	148	148	147	146	145
1889	6.88	6.81	6.78	6.76	6.72	6.68	6.72	6.78	6.82	6.87	6.89	6.88	142	140	138	137	136	135	136	137	137	137	136	135
1890	6.90	6.97	7.03	7.09	7.14	7.19	7.19	7.19	7.24	7.31	7.47	7.63	134	135	135	135	136	138	138	138	139	141	145	149
1891	7.76	7.90	8.02	8.18	8.34	8.51	8.67	8.84	8.95	9.01	9.11	9.18	153	153	153	163	168	173	176	179	184	190	196	201
1892	9.25	9.37	9.52	9.60	9.57	9.58	9.63	9.63	9.66	9.60	9.65	10.08	211	218	218	217	218	219	219	217	216	216	217	218
1893	10.18	10.25	10.30	10.32	10.37	10.42	10.38	10.31	10.26	10.30	10.25	10.14	230	220	223	225	225	225	226	227	227	226	229	233
1894	10.03	9.94	9.79	9.62	9.50	9.45	9.48	9.51	9.50	9.45	9.46	9.56	233	231	230	228	226	225	223	222	222	223	220	217
1895	9.65	9.50	9.52	9.55	9.61	9.65	9.69	9.73	9.73	9.68	9.51	9.23	216	212	208	207	206	204	203	203	202	198	196	191
1896	8.98	8.91	8.91	8.86	8.73	8.65	184	183	186	185	183	182

Table III.—Numbers expressing the Relative Sun-spot Frequency, as deduced by Dr. Wolf, and in the later years by Professor Wolfer.

Year.	Monthly values, as derived from observation.												Smoothed monthly values, each expressing an annual mean.											
	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1876	13.1	12.6	12.7	12.7	12.6	12.5	11.7	11.9	10.8	10.6	11.8	13.0
1877	3.3	6.0	7.8	0.1	5.8	6.4	0.1	0.0	5.3	1.1	4.1	0.5	6.6	6.0	5.3	4.6	4.0	3.4	3.3	3.0	2.4	2.3	8.0	7.1
1878	0.8	0.6	0.0	6.2	2.4	4.8	7.5	10.7	6.1	12.3	12.9	7.2	2.5	3.2	3.7	4.2	5.0	5.7	6.9	9.0	10.9	12.3	13.7	15.8
1879
1880	24.0	27.5	19.5	19.3	23.5	34.1	21.9	48.1	66.0	43.0	30.7	39.6	17.7	19.8	23.9	26.8	29.7	31.3	32.8	34.4	36.5	39.5	41.6	43.6
1881	36.4	53.2	51.5	51.7	43.5	60.5	76.9	58.0	53.2	64.0	54.8	47.3	46.9	49.7	49.6	49.9	51.8	54.2	54.6	55.6	57.0	59.5	62.9	62.4
1882	45.0	69.3	67.5	66.8	64.1	45.2	45.4	40.4	57.7	59.2	84.4	47.8	60.4	58.4	57.9	57.8	58.9	59.9	60.4	60.1	58.1	56.5	54.6	54.5
1883	60.6	46.9	42.8	82.1	32.1	76.5	80.6	46.0	82.6	83.8	84.5	75.9	57.3	59.0	59.0	59.8	60.8	62.3	65.0	67.9	71.4	73.0	74.2	74.6
1884	91.5	86.9	86.8	76.1	66.5	51.2	53.1	55.8	61.9	47.8	36.6	41.7	72.4	71.7	72.4	71.3	67.8	64.6	61.4	58.8	56.6	54.2	53.6	55.2
1885	42.8	71.8	49.8	55.0	73.0	83.7	66.5	50.0	39.6	38.7	33.3	27.7	57.1	57.4	56.2	54.9	54.4	53.2	51.6	49.2	47.6	47.4	46.3	41.1
1886	29.9	25.9	57.3	43.7	39.7	37.1	30.3	16.9	21.4	8.6	0.3	12.4	37.2	31.3	32.2	30.2	27.5	26.8	24.6	23.2	20.5	16.7	15.0	13.8
1887	10.3	13.2	4.2	6.9	20.0	15.7	23.3	21.4	7.4	6.6	6.9	20.7	13.1	13.0	12.6	11.9	12.1	12.7	13.1	13.0	12.9	13.0	12.4	11.4
1888	12.7	7.1	7.8	5.1	7.0	7.1	3.1	2.8	8.6	2.1	10.7	6.7	10.3	8.6	7.9	7.8	7.8	7.3	6.3	6.3	5.8	5.6	5.3	5.6
1889	0.8	8.5	7.0	4.3	2.4	6.4	9.7	20.6	6.5	2.1	0.2	6.1	5.6	6.6	7.2	7.1	6.7	6.3	6.5	6.3	5.9	5.7	5.7	5.6
1890	5.3	0.6	5.1	1.6	4.8	1.3	11.6	8.5	17.2	11.2	9.6	7.8	5.5	5.0	5.0	5.8	6.6	7.0	7.4	8.6	9.8	10.8	13.1	16.5
1891	13.5	22.2	10.4	20.5	41.1	48.3	58.8	35.2	53.8	51.5	41.9	33.2	20.5	23.5	26.0	29.2	32.2	34.6	37.9	42.5	46.3	50.0	53.7	56.5
1892	69.1	75.6	49.9	69.6	79.6	76.3	76.8	101.4	82.8	70.5	65.4	78.5	58.4	60.0	65.2	66.4	68.1	71.3	73.4	73.9	75.3	76.3	77.0	76.7
1893	75.0	73.0	65.7	89.1	84.7	88.2	88.8	129.2	77.9	79.7	75.1	83.5	78.0	79.7	81.5	82.5	83.3	84.3	85.3	86.1	86.0	85.2	86.9	86.7
1894	83.2	84.6	52.3	81.6	101.2	98.9	106.0	70.3	65.9	75.5	56.6	60.0	87.9	86.2	83.2	82.5	81.6	79.4	77.2	75.9	75.3	75.4	73.8	71.3
1895	63.3	67.2	61.0	76.9	67.5	71.5	47.8	68.9	57.7	67.9	47.2	70.2	67.7	65.2	64.8	64.2	63.5	63.5	62.5	60.7	59.9	58.2	55.1	52.5
1896	29.0	57.4	52.0	43.8	27.7	49.0	45.0	27.2	61.3	58.4	38.6	43.6	51.5	49.6	48.0	46.5	44.5	43.0

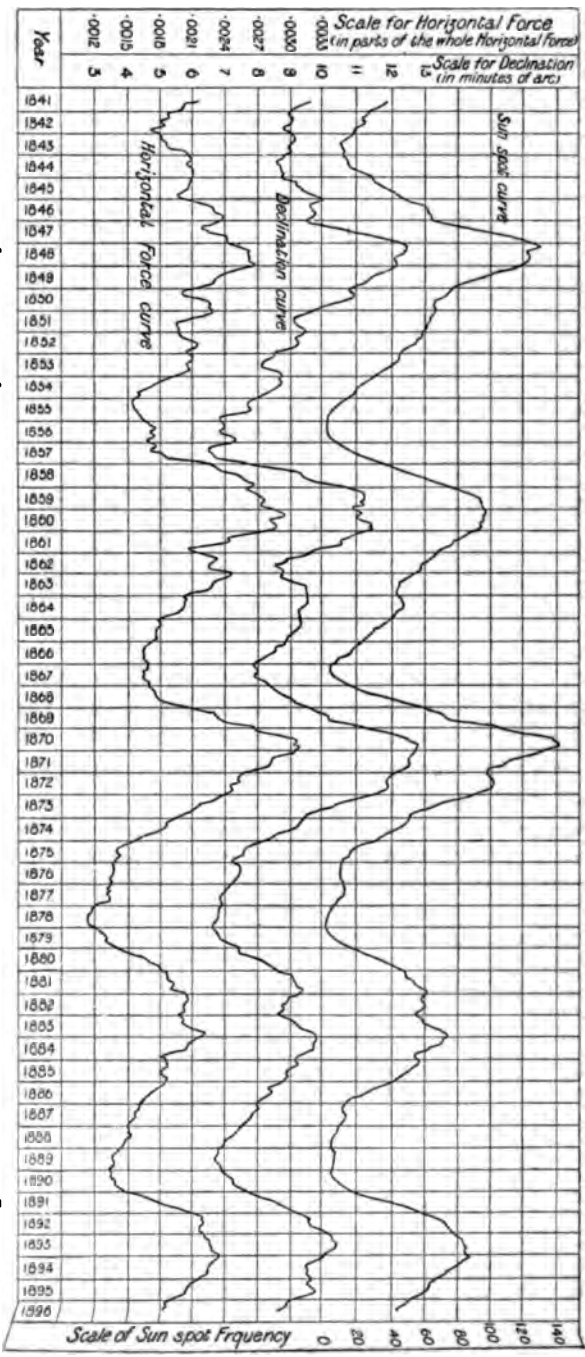
Table IV.—Monthly Mean Diurnal Range of Declination and Horizontal Force as determined from the observations on five selected quiet days in each month.

Year.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Declination.												
1889	2.9	4.7	6.4	9.6	8.3	8.4	8.0	8.0	7.5	6.1	4.3	3.2
1890	3.9	5.0	6.7	9.3	8.3	8.5	9.4	9.5	6.6	5.9	4.4	3.9
1891	4.0	4.3	9.1	8.7	11.4	10.3	10.9	9.8	10.1	9.0	6.7	3.9
1892	6.4	7.1	9.7	10.2	13.0	13.4	12.6	13.2	10.9	9.5	4.9	6.1
1893	7.0	6.6	11.4	12.7	14.1	12.0	13.0	12.2	11.7	9.2	7.5	4.2
1894	6.0	8.6	10.3	13.1	12.5	10.3	10.9	12.2	8.9	6.3	5.3	3.9
1895	2.9	6.9	9.5	11.9	10.9	14.6	11.8	11.9	9.6	6.9	4.0	3.9
1896	7.2	5.3	9.1	10.9	9.5	9.5	9.7	9.4	11.0	6.9	4.0	3.6
Horizontal Force.												
1889	42	79	103	177	192	202	185	235	183	155	101	59
1890	66	102	111	182	186	219	196	203	158	166	76	60
1891	101	106	155	183	264	280	254	267	244	217	162	63
1892	171	164	205	260	270	268	285	304	227	205	153	109
1893	118	181	240	287	287	289	308	280	272	226	201	120
1894	130	165	197	289	287	329	272	303	238	206	179	117
1895	72	189	202	318	284	305	292	251	209	195	117	61
1896	124	139	183	236	213	210	203	218	275	172	110	52

Table V.—Annual Means of the Monthly Mean Diurnal Range of Declination and Horizontal Force as determined from the observations on five selected quiet days in each month. (As in Table II, each number represents an annual mean of which the month itself is the middle point.)

Year.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Declination.												
1889	6.49	6.53	6.57	6.57	6.56	6.56
1890	6.62	6.75	6.77	6.73	6.72	6.75	6.79	6.76	6.83	6.91	7.01	7.22
1891	7.35	7.43	7.59	7.86	8.09	8.18	8.28	8.50	8.64	8.73	8.86	9.05
1892	9.25	9.47	9.64	9.70	9.64	9.66	9.77	9.78	9.83	10.00	10.15	10.14
1893	10.10	10.08	10.07	10.09	10.18	10.21	10.09	10.13	10.17	10.14	10.09	9.96
1894	9.80	9.71	9.59	9.35	9.14	9.04	8.90	8.70	8.59	8.51	8.39	8.50
1895	8.72	8.75	8.76	8.82	8.81	8.78	8.76	9.07	8.99	8.93	8.83	8.56
1896	8.26	8.07	8.03	8.08	8.06	8.05
Horizontal Force.												
1889	144	146	147	147	148	148
1890	149	148	146	145	148	144	145	147	149	151	154	160
1891	165	170	176	182	187	191	194	200	204	209	213	212
1892	213	215	216	215	214	216	215	214	216	219	220	222
1893	225	225	226	228	231	234	235	234	232	230	230	232
1894	232	232	231	229	227	226	224	222	223	225	226	225
1895	225	223	220	218	215	210	210	210	207	203	197	190
1896	182	177	178	180	179	178

Smoothed Curves of Sun-spot Frequency (Wolf), compared with corresponding Curves showing the Variation in Diurnal Range of the Magnetic Elements of Declination and Horizontal Force from Observations made at the Royal Observatory, Greenwich.

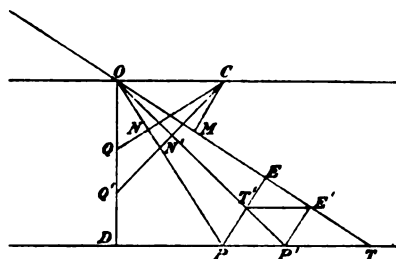


“On the Relative Retardation between the Components of a Stream of Light produced by the Passage of the Stream through a Crystalline Plate cut in any Direction with respect to the Faces of the Crystal.” By JAMES WALKER, M.A. Communicated by Professor R. B. CLIFTON, F.R.S. Received February 29—Read March 10, 1898.

The relative retardation between the components of a stream of light produced by the passage through a crystalline plate can, as is well known, only be determined in finite terms in a limited number of special cases. In general it is necessary to be content with an approximate solution, and those hitherto published have, as far as I have ascertained, never been carried beyond terms of the second order with respect to the sine of the angle of incidence of the light, while they do not readily lend themselves to a further approximation. The following method of dealing with the problem, which, except for the labour of calculation, can easily be extended to terms of any order, may then be of practical use, and, as leading to an interesting relation between the corresponding terms in the development of the roots of a certain biquadratic equation, of some interest.

1. Let the plane of the paper represent the plane of incidence, OT represent the normal to the front of the incident plane wave, OP and OP' the normals to the fronts of the two corresponding refracted waves; then if OM, ON, and ON' represent the spaces these waves would traverse in unit time, planes through M, N, and N' perpendicular to OT, OP, and OP' respectively will represent the positions that the fronts of the waves would occupy in unit time after leaving O, and these planes will, by Huygens' principle, intersect the surface of the crystalline plate in one straight line, projected on the plane of the figure in the point C.

Let OT, OP, and OP' meet the second surface of the plate in the



points T, P, and P' respectively, and through P and P' draw PE, P'E' perpendicular to OT, meeting it in the points E and E'; through E' draw E'T' parallel to the surface of the plate, meeting PE in T'.

Draw OD normal to the plate, meeting the second surface in D, and let the wave fronts CN and CN' meet this normal in the points Q and Q'.

Then the triangles OPD, CON, CQO are similar, as are also the triangles OP'D, CON', CQ'O, and the triangles T'E'E, COM.

The relative retardation, measured in time, of the two waves after both have traversed the plate, is represented by

$$\begin{aligned} \left[\frac{OP}{ON} - \frac{OE}{OM} \right] - \left[\frac{OP'}{ON'} - \frac{OE'}{OM} \right] &= \frac{OP}{ON} - \frac{OP'}{ON'} + \frac{EE'}{OM} \\ &= \frac{OC}{ON^2} DP - \frac{OC}{ON'^2} DP' + \frac{PP'}{OC} = \frac{CN^2}{OC \cdot ON^2} DP - \frac{CN'^2}{OC \cdot ON'^2} DP' \\ &= \frac{OD^2}{OC} \left[\frac{1}{DP} - \frac{1}{DP'} \right] = OD \left[\frac{1}{OQ} - \frac{1}{OQ'} \right]. \end{aligned}$$

Hence if the axis of Z be normal to the plate, and

$$lX + mY + n_1Z = 1 \qquad lX + mY + n_2Z = 1$$

are the equations of the refracted waves in unit time after passing through O, their relative retardation, measured in length in air, after both have traversed the plate is

$$\Delta = vT(n_1 - n_2),$$

where v is the propagational speed in air, and T is the thickness of the plate.

Also i being the angle of incidence, and w the azimuth of the plane of incidence with respect to that of XZ,

$$l = \sin i \cos w/v, \qquad m = \sin i \sin w/v.$$

2. In applying this result to the case of a plate cut in any manner from a biaxial crystal, let the surface of the plate on which the light is incident be taken as the plane of XY, the plate lying on the side of Z positive, and let the plane of XZ be taken so as to contain the least axis of elasticity of the crystal.

Let Ox , Oy , Oz be the axes of elasticity, and the angle $yoY = \phi$, the angle $zoZ = \chi$, then the transformation from the axes of elasticity to the new axes may be effected by the following successive transformations, each in one plane:—

- (1) Through an angle ϕ , in the plane of xy , from Ox , Oy to Ox_1 , Oy_1 .
- (2) Through an angle χ , in the plane zx_1 , from Oz , Ox_1 to OZ , OX .

The formulæ for these transformations are—

$$\begin{aligned}x &= x_1 \cos \phi - Y \sin \phi, & y &= x_1 \sin \phi + Y \cos \phi, \\x_1 &= X \cos \chi + Z \sin \chi, & z &= -X \sin \chi + Z \cos \chi,\end{aligned}$$

from which we obtain

$$\begin{aligned}x &= X \cos \phi \cos \chi - Y \sin \phi + Z \cos \phi \sin \chi, \\y &= X \sin \phi \cos \chi + Y \cos \phi + Z \sin \phi \sin \chi, \\z &= -X \sin \chi + Z \cos \chi.\end{aligned}$$

Now the equation to the wave surface referred to the axes of elasticity is

$$\frac{a^2 x^2}{r^2 - a^2} + \frac{b^2 y^2}{r^2 - b^2} + \frac{c^2 z^2}{r^2 - c^2} = 0,$$

a, b, c being the principal wave velocities, and the condition that the plane

$$lx + my + nz = 1$$

should be a tangent plane to it, is obtained by eliminating p between the equations

$$p^2 = (l^2 + m^2 + n^2)^{-1} \quad \text{and} \quad \frac{l^2}{a^2 - p^2} + \frac{m^2}{b^2 - p^2} + \frac{n^2}{c^2 - p^2} = 1.$$

Hence the condition that in the new system of co-ordinates the plane

$$lX + mY + nZ = 1$$

should touch the wave surface is found by eliminating p between the equations

$$p^2 = (l^2 + m^2 + n^2)^{-1},$$

$$\begin{aligned}& \frac{(l \cos \phi \cos \chi - m \sin \phi + n \cos \phi \sin \chi)^2}{a^2 - p^2} \\& + \frac{(l \sin \phi \cos \chi + m \cos \phi + n \sin \phi \sin \chi)^2}{b^2 - p^2} + \frac{(l \sin \chi - n \cos \chi)^2}{c^2 - p^2} = 0 \dots (i).\end{aligned}$$

The result of this elimination is a biquadratic in n , which, from the nature of the problem, has two real positive and two real negative roots, and if n_1, n_2 are the positive roots of the biquadratic, the relative retardation required is

$$\Delta = vT(n_1 - n_2).$$

3. Before proceeding with the general case, these results may be applied to certain *simple cases* :—

82 Mr. J. Walker. *Relative Retardation between Components*

(1) Let the plate be cut from a uniaxial crystal, then writing $b = a$, equation (i) gives the two equations

$$p^2 = a^2 \text{ and}$$

$$(p^2 - c^2) \{ (l \cos \chi + n \sin \chi)^2 + m^2 \} + (p^2 - a^2) (l \sin \chi - n \cos \chi)^2 = 0,$$

and the values of n are given by

$$a^2(l^2 + m^2 + n^2) = 1,$$

$$c^2(l^2 + m^2 + n^2) - 1 + (a^2 - c^2) (l \sin \chi - n \cos \chi)^2 = 0,$$

whence

$$n_1 = \frac{1}{a} \sqrt{1 - a^2 \frac{\sin^2 i}{v^2}},$$

$$n_2 = \left\{ \sqrt{(a^2 \cos^2 \chi + c^2 \sin^2 \chi) (1 - c^2 \sin^2 i / v^2) - c^2 (a^2 - c^2) \sin^2 \chi \cos^2 w \sin^2 i / v^2} \right. \\ \left. + (a^2 - c^2) \sin \chi \cos \chi \cos w \sin i / v \right\} \div (a^2 \cos^2 \chi + c^2 \sin^2 \chi)$$

and

$$\frac{\Delta}{T} = \frac{\sqrt{v^2 - a^2 \sin^2 i}}{a} - \frac{(a^2 - c^2) \sin \chi \cos \chi \cos w \sin i}{a^2 \cos^2 \chi + c^2 \sin^2 \chi} \\ - \frac{\sqrt{(a^2 \cos^2 \chi + c^2 \sin^2 \chi) (v^2 - c^2 \sin^2 i) - c^2 (a^2 - c^2) \sin^2 \chi \cos^2 w \sin^2 i}}{a^2 \cos^2 \chi + c^2 \sin^2 \chi}.$$

(2) Let the plate be cut from a biaxial crystal perpendicularly to the mean line; then taking the axes of elasticity as the co-ordinate axes, the biquadratic in n becomes

$$(b^2 c^2 l^2 + c^2 a^2 m^2 + a^2 b^2 n^2) (l^2 + m^2 + n^2) \\ - \{ (b^2 + c^2) l^2 + (c^2 + a^2) m^2 + (a^2 + b^2) n^2 \} + 1 = 0,$$

or

$$a^2 b^2 n^4 - \{ (a^2 + b^2) - b^2 (c^2 + a^2) l^2 - a^2 (b^2 + c^2) m^2 \} n^2 \\ + \{ 1 - c^2 (l^2 + m^2) \} \{ 1 - b^2 l^2 - a^2 m^2 \} = 0,$$

and n_1, n_2 being the positive roots of this equation

$$(n_1 - n_2)^2 a^2 b^2 = (a^2 + b^2) - b^2 (c^2 + a^2) l^2 - a^2 (b^2 + c^2) m^2 \\ - 2ab \sqrt{\{ 1 - c^2 (l^2 + m^2) \} \{ 1 - b^2 l^2 - a^2 m^2 \}},$$

and

$$a^2 b^2 \frac{\Delta^2}{T^2} = (a^2 + b^2) v^2 - \{ b^2 (c^2 + a^2) \cos^2 w + a^2 (b^2 + c^2) \sin^2 w \} \sin^2 i \\ - 2ab \sqrt{(v^2 - c^2 \sin^2 i) \{ v^2 - (b^2 \cos^2 w + a^2 \sin^2 w) \sin^2 i \}},$$

4. Returning to the general equation (i), write

$$\begin{aligned} A' &= \cos \phi \sin \chi, & A &= \cos \phi \cos \chi \cos w - \sin \phi \sin w, \\ B' &= \sin \phi \sin \chi, & B &= \sin \phi \cos \chi \cos w + \cos \phi \sin w, \\ C' &= \cos \chi, & C &= -\sin \chi \cos w. \end{aligned}$$

Then the equations between which p has to be eliminated become

$$\begin{aligned} \left(A'n + A \frac{\sin i}{v} \right)^2 \left(\frac{b^2}{p^2} - 1 \right) \left(\frac{c^2}{p^2} - 1 \right) &+ \left(B'n + B \frac{\sin i}{v} \right)^2 \left(\frac{c^2}{p^2} - 1 \right) \left(\frac{a^2}{p^2} - 1 \right) \\ &+ \left(C'n + C \frac{\sin i}{v} \right)^2 \left(\frac{a^2}{p^2} - 1 \right) \left(\frac{b^2}{p^2} - 1 \right) = 0. \end{aligned}$$

$$\frac{1}{p^2} = n^2 + \frac{\sin^2 i}{v^2},$$

and since

$$\left(A'n + A \frac{\sin i}{v} \right)^2 + \left(B'n + B \frac{\sin i}{v} \right)^2 + \left(C'n + C \frac{\sin i}{v} \right)^2 = n^2 + \frac{\sin^2 i}{v^2} = \frac{1}{p^2},$$

the result of the elimination becomes

$$\begin{aligned} \left(A'^2 n^2 + 2AA' \frac{\sin i}{v} n + A^2 \frac{\sin^2 i}{v^2} \right) &\left(b^2 c^2 n^2 + b^2 c^2 \frac{\sin^2 i}{v^2} - b^2 - c^2 \right) \\ &+ \left(B'^2 n^2 + 2BB' \frac{\sin i}{v} n + B^2 \frac{\sin^2 i}{v^2} \right) \left(c^2 a^2 n^2 + c^2 a^2 \frac{\sin^2 i}{v^2} - c^2 - a^2 \right) \\ &+ \left(C'^2 n^2 + 2CC' \frac{\sin i}{v} n + C^2 \frac{\sin^2 i}{v^2} \right) \left(a^2 b^2 n^2 + a^2 b^2 \frac{\sin^2 i}{v^2} - a^2 - b^2 \right) + 1 = 0; \end{aligned}$$

and multiplying out and arranging the terms, this equation may be written

$$\begin{aligned} n^4 - 2d_1 \frac{\sin i}{v} n^3 + \left(c_0 + c_2 \frac{\sin^2 i}{v^2} \right) n^2 - 2 \left(b_1 \frac{\sin i}{v} + b_3 \frac{\sin^3 i}{v^3} \right) n &+ a_0 + a_2 \frac{\sin^2 i}{v^2} \\ &+ a_4 \frac{\sin^4 i}{v^4} = 0 \dots \text{(ii)}, \end{aligned}$$

where

$$\begin{aligned} a_0 &= \frac{1}{\Sigma A'^2 b^2 c^2}, & a_2 &= -\frac{\Sigma A^2 (b^2 + c^2)}{\Sigma A'^2 b^2 c^2}, & a_4 &= \frac{\Sigma A^2 b^2 c^2}{\Sigma A'^2 b^2 c^2}, \\ b_1 &= \frac{\Sigma A A' (b^2 + c^2)}{\Sigma A'^2 b^2 c^2}, & b_3 &= d_1 = -\frac{\Sigma A A' b^2 c^2}{\Sigma A'^2 b^2 c^2}, \\ c_0 &= -\frac{\Sigma A'^2 (b^2 + c^2)}{\Sigma A'^2 b^2 c^2}, & c_2 &= 1 + a_4. \end{aligned}$$

84 Mr. J. Walker. *Relative Retardation between Components*

Now the roots of equation (ii), p, q, r, s will be functions of $\sin i/v$, and expanding these in series proceeding according to powers of this quantity, we may write generally

$$p = p_0 + p_1 \frac{\sin i}{v} + p_2 \frac{\sin^2 i}{v^2} + \dots$$

$$q = q_0 + q_1 \frac{\sin i}{v} + q_2 \frac{\sin^2 i}{v^2} + \dots$$

$$r = r_0 + r_1 \frac{\sin i}{v} + r_2 \frac{\sin^2 i}{v^2} + \dots$$

$$s = s_0 + s_1 \frac{\sin i}{v} + s_2 \frac{\sin^2 i}{v^2} + \dots$$

But

$$\Sigma p = 2d_1 \frac{\sin i}{v}, \quad \Sigma pq = c_0 + c_2 \frac{\sin^2 i}{v^2}, \quad \Sigma pqr = 2b_1 \frac{\sin i}{v} + 2b_3 \frac{\sin^3 i}{v^3},$$

$$\Sigma pqrs = a_0 + a_2 \frac{\sin^2 i}{v^2} + a_4 \frac{\sin^4 i}{v^4},$$

and therefore equating the coefficients of like powers of $\sin i/v$ on the two sides of these equations, the coefficients p, q, r, s may in general be determined in succession by means of linear equations, provided we can determine p_0, q_0, r_0, s_0 , which may be at once done, since they are the roots of the equation

$$n^4 + c_0 n^2 + a_0 = 0.$$

Moreover, when this method is practicable, the roots of the biquadratic take the following form

$$\pi_1 + \rho_1, \quad -\pi_1 + \rho_1, \quad \pi_2 + \rho_2, \quad -\pi_2 + \rho_2,$$

where

$$\pi_1 = \alpha_0 + \alpha_2 \frac{\sin^2 i}{v^2} + \alpha_4 \frac{\sin^4 i}{v^4} + \dots,$$

$$\pi_2 = \beta_0 + \beta_2 \frac{\sin^2 i}{v^2} + \beta_4 \frac{\sin^4 i}{v^4} + \dots,$$

$$\rho_1 = \gamma_1 \frac{\sin i}{v} + \gamma_3 \frac{\sin^3 i}{v^3} + \gamma_5 \frac{\sin^5 i}{v^5} + \dots,$$

$$\rho_2 = \delta_1 \frac{\sin i}{v} - \gamma_3 \frac{\sin^3 i}{v^3} - \gamma_5 \frac{\sin^5 i}{v^5} + \dots$$

For suppose that this is true as far as the terms involving $\sin^{n-1} i/v^{n-1}$, and let $\alpha, \beta, \gamma, \delta$ denote the sum of the terms in $\pi_1, \pi_2, \rho_1, \rho_2$ respectively of an order less than n , then we may write

$$p = \alpha + \gamma + \mu, \quad q = -\alpha + \gamma + \mu_2, \quad r = \beta + \delta + \mu_3, \quad s = -\beta + \delta + \mu_4,$$

and neglecting the products of the μ 's with one another, and with γ and δ , since such products will introduce terms that we shall not require, we have

$$\Sigma p = \mu_1 + \mu_2 + \mu_3 + \mu_4 + 2(\gamma + \delta),$$

$$\Sigma pq = \alpha(\mu_2 - \mu_1) + \beta(\mu_4 - \mu_3) - (\alpha^2 + \beta^2) + (\gamma + \delta)^2 + 2\gamma\delta,$$

$$\Sigma pqr = -\alpha^2(\mu_3 + \mu_4) - \beta^2(\mu_1 + \mu_2) - 2\alpha^2\delta - 2\beta^2\gamma + 2\gamma\delta(\gamma + \delta),$$

$$\Sigma pqrs = -\alpha\beta\{\alpha(\mu_4 - \mu_3) + \beta(\mu_2 - \mu_1)\} + \alpha^2\beta^2 - \alpha^2\delta^2 - \beta^2\gamma^2 + \gamma^2\delta^2.$$

Hence if p_n, q_n, r_n, s_n denote the coefficients of \sin^ni/v^n in p, q, r, s respectively, and $[]_n$ denote the coefficient of \sin^ni/v^n in the expression within the vinculum, the equations that determine these quantities are—

(1) If $n = 2m$

$$p_{2m} + q_{2m} + r_{2m} + s_{2m} = 0,$$

$$\alpha_0(q_{2m} - p_{2m}) + \beta_0(s_{2m} - r_{2m}) = c_{2m} + [\alpha^2 + \beta^2 - (\gamma + \delta)^2 - 2\gamma\delta]_{2m} = R_{2m}, \text{ say,}$$

$$\beta_0^2(p_{2m} + q_{2m}) + \alpha_0^2(r_{2m} + s_{2m}) = 0,$$

$$-\beta_0(q_{2m} - p_{2m}) - \alpha_0(s_{2m} - r_{2m}) = \frac{1}{\alpha_0\beta_0}\{a_{2m} - [\alpha^2\beta^2 - \alpha^2\delta^2 - \beta^2\gamma^2 + \gamma^2\delta^2]_{2m}\} = S_{2m}, \text{ say,}$$

and from the first and third of these equations, we have, *unless* $\alpha_0^2 = \beta_0^2$,

$$p_{2m} + q_{2m} = r_{2m} + s_{2m} = 0,$$

which relations hold whatever m may be.

(2) If $n = 2m + 1$,

$$p_{2m+1} + q_{2m+1} + r_{2m+1} + s_{2m+1} = 2d_{2m+1},$$

$$\alpha_0(q_{2m+1} - p_{2m+1}) + \beta_0(s_{2m+1} - r_{2m+1}) = 0,$$

$$-\beta_0^2(p_{2m+1} + q_{2m+1}) - \alpha_0^2(r_{2m+1} + s_{2m+1}) = 2b_{2m+1} + 2[\alpha^2\delta + \beta^2\gamma - \gamma\delta(\gamma + \delta)]_{2m+1} = T_{2m+1}, \text{ say,}$$

$$\beta_0(q_{2m+1} - p_{2m+1}) + \alpha_0(s_{2m+1} - r_{2m+1}) = 0,$$

and from the second and fourth of these equations

$$q_{2m+1} = p_{2m+1}, \quad s_{2m+1} = r_{2m+1}, \text{ unless } \alpha_0^2 = \beta_0^2;$$

and when $m > 0$, $d_{2m+1} = 0$, so that $r_{2m+1} = p_{2m+1}$ ($m > 0$).

86 Mr. J. Walker. *Relative Retardation between Components*

Leaving then for the present the case in which $\alpha_0^2 = \beta_0^2$, the roots have the form given above, and

$$\gamma_1 = \frac{\alpha_0^2 d_1 + b_1}{\alpha_0^2 - \beta_0^2}, \quad \delta_1 = -\frac{\beta_0^2 d_1 + b_1}{\alpha_0^2 - \beta_0^2}, \quad \gamma_{2m+1} = \frac{T_{2m+1}}{\alpha_0^2 - \beta_0^2},$$

$$\alpha_{2m} = -\frac{1}{2} \frac{\alpha_0 R_{2m} + \beta_0 S_{2m}}{\alpha_0^2 - \beta_0^2}, \quad \beta_{2m} = \frac{1}{2} \frac{\beta_0 R_{2m} + \alpha_0 S_{2m}}{\alpha_0^2 - \beta_0^2},$$

whence

$$\frac{\Delta}{vT} = \pi_1 - \pi_2 + \rho_1 - \rho_2$$

$$= \alpha_0 - \beta_0 + \frac{2b_1 + (\alpha_0^2 + \beta_0^2) d_1}{\alpha_0^2 - \beta_0^2} \cdot \frac{\sin i}{v} - \frac{1}{2} \frac{R_2 + S_2}{\alpha_0 - \beta_0} \cdot \frac{\sin^2 i}{v^2}$$

$$+ 2 \frac{T_2}{\alpha_0^2 - \beta_0^2} \cdot \frac{\sin^3 i}{v^3} - \frac{1}{2} \frac{R_4 + S_4}{\alpha_0 - \beta_0} \cdot \frac{\sin^4 i}{v^4} + \dots,$$

the terms of which may be readily calculated in succession.

5. It is now necessary to consider the case reserved above, in which $\alpha_0^2 = \beta_0^2$. When this occurs, the two refracted waves, corresponding to normal incidence of the light, traverse the plate with the same velocity, and hence the plate is perpendicular to an optic axis. The case, then, of a plate cut perpendicularly to an optic axis requires a special investigation.

Now for a plate so cut—

$$\phi = 0, \quad \cos \chi = \sqrt{(b^2 - c^2)/\sqrt{(a^2 - c^2)}}, \quad \sin \chi = \sqrt{(a^2 - b^2)/\sqrt{(a^2 - c^2)}},$$

and the biquadratic (ii) has the form

$$n^4 - 2d_1' \frac{\sin i}{v} \cdot n^3 + \left(c_0' + c_2' \frac{\sin^2 i}{v^2} \right) n^2 - 2 \left(b_1' \frac{\sin i}{v} + b_3' \frac{\sin^3 i}{v^3} \right) n$$

$$+ a_0' + a_2' \frac{\sin^2 i}{v^2} + a_4' \frac{\sin^4 i}{v^4} = 0,$$

where

$$d_1' = b_3' = \cos w \cdot \frac{1}{b^2} \sqrt{(a^2 - b^2)(b^2 - c^2)},$$

$$b_1' = -\cos w \cdot \frac{1}{b^4} \sqrt{(a^2 - b^2)(b^2 - c^2)},$$

$$c_0' = -\frac{2}{b^3}, \quad c_2' = 1 + a_4' = 1 + \frac{a^2 c^2}{b^4} + \frac{1}{b^4} (a^2 - b^2)(b^2 - c^2) \cos^2 w,$$

$$a_0' = \frac{1}{b^4}, \quad a_2' = -\frac{a^2 + c^2}{b^4},$$

whence writing $n + \frac{1}{2}d_1' \frac{\sin i}{v}$ for n , the equation becomes

$$n^4 + \left(c_0 + c_2 \frac{\sin^2 i}{v^2}\right) n^2 - 2b_2 \frac{\sin^3 i}{v^3} \cdot n + a_0 + a_2 \frac{\sin^2 i}{v^2} + a_4 \frac{\sin^4 i}{v^4} = 0,$$

where

$$\begin{aligned} c_0 &= -\frac{2}{b^2}, & c_2 &= \frac{1}{b^4} \{b^4 + a^2 c^2 - \frac{1}{2}(a^2 - b^2)(b^2 - c^2) \cos^2 w\}, \\ b_2 &= \frac{1}{2} \cdot \frac{b^4 - a^2 c^2}{b^6} \sqrt{(a^2 - b^2)(b^2 - c^2)} \cdot \cos w, \\ a_0 &= \frac{1}{b^4}, & a_2 &= -\frac{1}{b^4} \left\{a^2 + c^2 - \frac{1}{2} \frac{(a^2 - b^2)(b^2 - c^2)}{b^2} \cos^2 w\right\}, \\ a_4 &= \frac{1}{b^4} \left\{a^2 c^2 + \frac{1}{4} \cdot \frac{b^4 + a^2 c^2}{b^4} (a^2 - b^2)(b^2 - c^2) \cos^2 w \right. \\ &\quad \left. + \frac{1}{16} \cdot \frac{(a^2 - b^2)^2 (b^2 - c^2)^2}{b^4} \cos^4 w\right\}. \end{aligned}$$

Let us, in the first place, neglect the coefficient of n , then we have—

$$\begin{aligned} n^2 &= \\ &\frac{1}{2} \left\{ -c_0 - c_2 \frac{\sin^2 i}{v^2} \pm \sqrt{c_0^2 - 4a_0 + 2(c_0 c_2 - 2a_2) \frac{\sin^2 i}{v^2} + (c_2^2 - 4a_4) \frac{\sin^4 i}{v^4}} \right\} \\ &= \frac{1}{2b^4} \left[2b^2 - \{b^4 + a^2 c^2 - \frac{1}{2}(a^2 - b^2)(b^2 - c^2) \cos^2 w\} \frac{\sin^2 i}{v^2} \right. \\ &\quad \left. \pm 2b \sqrt{(a^2 - b^2)(b^2 - c^2)} \right. \\ &\quad \left. \times \frac{\sin i}{v} \sqrt{1 + \left\{ \frac{(b^4 - a^2 c^2)^2}{4b^2(a^2 - b^2)(b^2 - c^2)} - \frac{1}{2} \frac{b^4 + a^2 c^2}{b^2} \cos^2 w \right\} \frac{\sin^2 i}{v^2}} \right], \end{aligned}$$

and the roots are $\pm (\pi + \rho)$, $\pm (\pi - \rho)$, where

$$\begin{aligned} \pi &= p_0 + p_2 \frac{\sin^2 i}{v^2} + p_4 \frac{\sin^4 i}{v^4} + \dots \\ \rho &= p_1 \frac{\sin i}{v} + p_3 \frac{\sin^3 i}{v^3} + \dots \end{aligned}$$

and writing for shortness

$$\begin{aligned} P &= \sqrt{(a^2 - b^2)(b^2 - c^2)}, & Q &= \frac{1}{4}(a^2 - b^2)(b^2 - c^2) \cos^2 w - \frac{1}{4}(b^4 + a^2 c^2), \\ R &= \frac{1}{4} \frac{(b^4 - a^2 c^2)^2}{(a^2 - b^2)(b^2 - c^2)} - \frac{1}{2}(b^4 + a^2 c^2) \cos^2 w, \end{aligned}$$

we have

88 Mr. J. Walker. *Relative Retardation between Components*

$$p_0 = \frac{1}{b}, \quad p_1 = \frac{P}{2b^2}, \quad p_2 = \frac{1}{2b^3} (Q - \frac{1}{4}P^2), \quad p_3 = \frac{1}{4b^4} (PR - PQ + \frac{1}{4}P^3),$$

$$p_4 = \frac{1}{8b^5} (\frac{1}{2}P^2Q - Q^2 - P^2R - \frac{5}{16}P^4), \dots$$

Suppose now that the actual roots of the biquadratic are—

$$\pi + \rho + \alpha, \quad -\pi - \rho + \beta, \quad \pi - \rho + \gamma, \quad -\pi + \rho + \delta;$$

then

$$\alpha + \beta + \gamma + \delta = 0,$$

$$(\pi + \rho)(\beta - \alpha) + (\pi - \rho)(\delta - \gamma) + \alpha\beta + \gamma\delta + (\alpha + \beta)(\gamma + \delta) = 0,$$

$$(\pi + \rho + \alpha)(-\pi - \rho + \beta)(\gamma + \delta)$$

$$+ (\pi - \rho + \gamma)(-\pi + \rho + \delta)(\alpha + \beta) = 2b_3 \frac{\sin^3 i}{v^3},$$

$$\alpha\beta\gamma\delta + (\pi + \rho)\gamma\delta(\beta - \alpha) + (\pi - \rho)\alpha\beta(\delta - \gamma)$$

$$+ (\pi^2 - \rho^2)(\beta - \alpha)(\delta - \gamma) - (\pi + \rho)^2\{\gamma\delta + (\pi - \rho)(\delta - \gamma)\}$$

$$- (\pi - \rho)^2\{\alpha\beta + (\pi + \rho)(\beta - \alpha)\} = 0,$$

which, on reducing and simplifying, become

$$\alpha + \beta + \gamma + \delta = 0,$$

$$4\pi(\alpha + \gamma) + 4\rho(\alpha + \delta) + \alpha^2 + \beta^2 + \gamma^2 + \delta^2 = 0,$$

$$(\alpha + \beta)\{4\pi\rho + 2(\pi + \rho)(\alpha - \beta) + \alpha^2 + \beta^2\} = 2b_3 \frac{\sin^3 i}{v^3},$$

$$-2\pi\rho\{2(\pi + \rho)(\alpha - \beta) + \alpha^2 + \beta^2\} + 2\pi^2(\beta\delta + \alpha\gamma) + 2\rho^2(\beta\gamma + \alpha\delta)$$

$$+ \pi(\alpha + \gamma)(\beta\delta + \alpha\gamma) + \rho(\alpha + \delta)(\beta\gamma + \alpha\delta) + \alpha\beta\gamma\delta = 0.$$

Let

$$\alpha = \alpha_2 \frac{\sin^3 i}{v^2} + \alpha_3 \frac{\sin^3 i}{v^3} + \alpha_4 \frac{\sin^4 i}{v^4} + \dots,$$

with similar expressions for β, γ, δ ; then the terms involving $\sin^2 i/v^2$ give—

$$\alpha_2 + \beta_2 + \gamma_2 + \delta_2 = 0, \quad \alpha_2 + \gamma_2 = 0,$$

$$\therefore \gamma_2 = -\alpha_2 \quad \text{and} \quad \delta_2 = -\beta_2,$$

and from the terms involving $\sin^3 i/v^3$, we obtain—

$$\alpha_3 + \beta_3 + \gamma_3 + \delta_3 = 0,$$

$$p_0(\alpha_3 + \gamma_3) + p_1(\alpha_2 + \delta_2) = 0,$$

$$2p_0p_1(\alpha_2 + \beta_2) = b_3,$$

$$\alpha_3 - \beta_3 = 0,$$

whence

$$\alpha_2 = \beta_2 = -\gamma_2 = -\delta_2 = b_2/(4p_0p_1) = \frac{b^3}{2P} \cdot b_3,$$

and

$$\gamma_3 = -\alpha_2, \quad \delta_3 = -\beta_2.$$

Again, from the terms involving $\sin^4 i/v^4$ —

$$\alpha_4 + \beta_4 + \gamma_4 + \delta_4 = 0,$$

$$p_0(\alpha_4 + \gamma_4) + p_1(\alpha_3 + \delta_3) + \alpha_2^2 = 0,$$

$$\alpha_3 + \beta_3 = 0,$$

$$p_1(\alpha_3 - \beta_3) + \alpha_2^2 = 0,$$

whence $\alpha_3 = -\beta_3 = -\gamma_3 = \delta_3 = -\frac{\alpha_2^2}{2p_1} = -\frac{b^3}{4P^2} \cdot b_3^2,$

and

$$\alpha_4 = -\gamma_4, \quad \beta_4 = -\delta_4.$$

Similarly the terms involving $\sin^4 i/v^5$ give—

$$\alpha_5 + \beta_5 + \gamma_5 + \delta_5 = 0,$$

$$p_0(\alpha_5 + \gamma_5) + p_1(\alpha_4 + \delta_4) = 0,$$

$$p_0p_1(\alpha_4 + \beta_4) + 2(p_1p_2 + p_0p_3)\alpha_2 + 2p_0\alpha_2\alpha_3 = 0,$$

$$p_0(\alpha_4 - \beta_4) + p_1(\alpha_3 - \beta_3) + \alpha_2^2 = 0, \quad \text{or} \quad \alpha_4 - \beta_4 = 0.$$

Hence

$$\alpha_4 = \beta_4 = -\gamma_4 = -\delta_4 = -\frac{p_1p_2 + p_0p_3}{p_0p_1} \frac{\alpha_2\alpha_3}{p_1} = -\frac{Rb}{4P} \cdot b_3 + \frac{b^{13}}{4P^2} \cdot b_3^3,$$

and

$$\alpha_5 = -\gamma_5, \quad \beta_5 = -\delta_5,$$

and so on.

Hence, as far as terms of the fourth order,

$$\alpha = -\gamma = \alpha_2 \frac{\sin^2 i}{v^2} + \alpha_3 \frac{\sin^3 i}{v^3} + \alpha_4 \frac{\sin^4 i}{v^4},$$

$$\beta = -\delta = \alpha_2 \frac{\sin^2 i}{v^2} - \alpha_3 \frac{\sin^3 i}{v^3} + \alpha_4 \frac{\sin^4 i}{v^4},$$

where

$$\alpha_2 = \frac{b^4 - a^2c^2}{4b^3} \cos w, \quad \alpha_3 = -\frac{(b^4 - a^2c^2)^2}{16b^4 \sqrt{(a^2 - b^2)(b^2 - c^2)}} \cos^2 w,$$

$$\alpha_4 = \frac{(b^4 - a^2c^2)}{32b^5(a^2 - b^2)(b^2 - c^2)} \{2(a^2 - b^2)(b^2 - c^2)(b^4 + a^2c^2) \cos^2 w - (b^4 - a^2c^2)^2 \sin^2 w\} \cos w,$$

90 *Relative Retardation between Components of a Stream of Light.*

and the difference of the positive roots being $2\rho + \alpha - \gamma$, we have, to the same degree of approximation,

$$\begin{aligned} \frac{\Delta}{vT} &= 2p_1 \frac{\sin i}{v} + 2\alpha_2 \frac{\sin^2 i}{v^2} + 2(p_3 + \alpha_3) \frac{\sin^3 i}{v^3} + 2\alpha_4 \frac{\sin^4 i}{v^4} \\ &= \frac{\sqrt{(a^2 - b^2)(b^2 - c^2)}}{b^2} \cdot \frac{\sin i}{v} + \frac{1}{2} \cdot \frac{b^4 - a^2 c^2}{b^3} \cos w \frac{\sin^2 i}{v^2} \\ &\quad + \frac{1}{8} \cdot \frac{(a^2 - c^2)^2}{\sqrt{(a^2 - b^2)(b^2 - c^2)}} \sin^2 w \frac{\sin^3 i}{v^3} + \frac{1}{8} \cdot \frac{b^4 - a^2 c^2}{b^4(a^2 - b^2)(b^2 - c^2)} \\ &\quad \times \{2(a^2 - b^2)(b^2 - c^2)(b^4 + a^2 c^2) \cos^2 w - (b^4 - a^2 c^2)^2 \sin^2 w\} \cos w \frac{\sin^4 i}{v^4}. \end{aligned}$$

6. The proposition on which the above investigation depends was first suggested to me by an analogous theorem given by McCullagh,* in connection with the surface of wave-slowness,† or, as he terms it, the surface of refraction or index surface; in fact, the one may be deduced from the other by reciprocating with respect to a sphere of unit radius concentric with the surfaces.

I have since found that Zech‡ has employed the same principle for the determination of the rings of biaxial crystals, but his method of dealing with the biquadratic equation is essentially different from that given above, and leads only to the determination of the terms of the second order.

My thanks are also due to Mr. J. L. S. Hatton for some useful suggestions that led me to the adoption of the above methods of approximation.

* 'Collected Works,' p. 46.

† The first pedal of the wave-surface is sometimes erroneously called the surface of wave-slowness; but, as Sir William Hamilton calls the inverse of the wave-velocity the wave-slowness, the inverse of this surface, or the polar reciprocal of the wave-surface, is properly the surface of wave-slowness. That this was the name given to the polar reciprocal of the wave-surface by Sir William Hamilton appears from Lloyd's "Report on Physical Optics" ('Collected Works,' p. 122), and from McCullagh ('Collected Works,' p. 96), though in his papers he calls it the surface of components of normal slowness.

‡ 'Pogg. Ann.,' vol. 97, p. 129; vol. 102, p. 354.

“An Extension of Maxwell's Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and Allied Phenomena.” By EDWIN EDSER, A.R.C.S. Communicated by Captain W. DE W. ABNEY, C.B., F.R.S. Received February 18,—Read March 10, 1898.

(Abstract.)

A dielectric, like an electrolyte, is assumed to consist of molecules, each comprising, in the simplest case, two oppositely charged atoms at a definite distance apart. In a homogeneous medium, when not subjected to electric strain, these molecules will be arranged in such a manner that any element of volume will possess no resultant electric moment. If a definite potential difference be maintained between any two parallel planes in the medium, the positively charged atoms will move to points of lower, and the negatively charged atoms to points of higher, potential. Thus two kinds of molecular strain are produced: firstly, a molecular rotation; and secondly, a separation in the molecule of the constituent atoms. Let P be the actual electromotive intensity at any point in the medium, and D be the electric displacement other than that produced by the atomic charges. Then

$$P(1+4\pi M) = 4\pi D,$$

where M is a constant depending on the nature of the medium. The quantity $1+4\pi M$ represents the specific inductive capacity of the medium. The actual linear displacement of the atoms is shown to be small when compared with molecular magnitudes.

Maxwell's equation, expressing that the line integral of the electromotive intensity round a closed circuit is equal to the rate of decrease of the magnetic induction through the circuit, needs no modification when the propagation of disturbances through the above medium is considered. Maxwell's second equation is modified by adding to the total displacement current at any point the expression Σqv_x , where q is the atomic charge, v_x is the velocity of that charge in the direction considered, and Σ denotes summation for unit volume.

Subsidiary equations for the atomic vibrations (rotational and separational) are given, and the refractive index is finally determined in the form

$$\mu^2 = \mu_\alpha^2 + \frac{c'\lambda_1^2}{\lambda^2 - \lambda_1^2} + \frac{c''\lambda_2^2}{\lambda^2 - \lambda_2^2},$$

which is the most general form of Ketteler's dispersion formula.

92 *Extension of Maxwell's Electro-magnetic Theory of Light.*

μ^2_{α} is found to be equal to the specific inductive capacity of the medium, as previously determined.

For a medium which might be compressed without altering the period of vibration of the constituent molecules, Gladstone and Dale's law, in the modified form $\mu^2 - 1 \propto$ density, would follow.

Double refraction, in case of a uniaxial crystal, is explained on the assumption that the molecules are arranged with their axes parallel to a certain direction. Electrical disturbances perpendicular to this direction will produce a molecular rotation, whilst those parallel to this direction will produce an inter-atomic separation. The doubly refracting nature of a dielectric when subjected to electric strain is thus explained; and it is pointed out that Lord Kelvin was led to postulate a crystalline structure similar to the above to account for the pyro-electric properties of tourmaline, &c.

For infinitely quick vibrations the refractive index of the above medium will be equal to unity, a result possibly explaining the action of material bodies on Röntgen radiations.

Assuming a metallic or quasi-metallic substance to have a structure essentially similar to that described above, with the addition that a viscous term is included in the equation for the atomic vibration, the refractive index of a metal is found in the form of a complex quantity, the imaginary part of which is essentially positive. The ordinary laws of metallic reflection, as deduced by Cauchy and others, will therefore hold. It is shown that for those metals in which the real part of the square of the refractive index is a large negative quantity, the velocity of propagation of light will be inversely proportional to the molecular viscosity. Since Mr. Tomlinson has shown that for those metals which he had examined the order of magnitude of the specific electrical resistances was the same as that of the molecular viscosities, a connection is established between the velocity of light and the electrical conductivity of a metal, agreeing with that obtained experimentally by Kundt.

The initial assumptions in the above investigation are similar to those made by Helmholtz in his papers on the "Electro-magnetic Theory of Dispersion."* Some doubt has been expressed as to whether Helmholtz's developments are in consonance with Maxwell's theory.† In the present case the principle of Least Action is not used. The dispersion formula obtained differs from that of Helmholtz, but bears a general resemblance to that obtained by Rieff in his modification of Helmholtz's theory.‡ A more definite physical significance is, however, given to the various constants introduced.

* 'Wied. Ann.,' vol. 48, pp. 389—405, 723—725.

† O. Heaviside, 'Electrician,' vol. 37, August 7, 1896.

‡ 'Wied. Ann.,' vol. 55, pp. 82—95.

CROONIAN LECTURE.—“The Nature and Significance of Functional Metabolism in the Plant” (*Das Wesen und die Bedeutung des Betriebsstoffwechsels in der Pflanze*). By WILHELM PFEFFER, Sc.D. (Cantab.), of the University of Leipzig, For. Mem. R.S. Received March 9,—Read March 17, 1898.

(Translation of Author's Abstract.)

The fact that a mould fungus will thrive in a solution, from which, with the exception of certain inorganic acids, it can obtain nothing but sugar, affords proof that the elaboration of these food substances in metabolism not only provides the numerous carbon compounds which are concerned in the construction of the plant, but also serves as a sufficient source of energy for the performance of its functions. For in the plant, as in the animal, vital activity comes to a standstill if the conditions and the energy necessary for the discharge of its functions are not constantly provided by means of profound chemical decompositions. Just as in animals, a great amount of internal and external work has to be accomplished, in order to carry on and maintain the action of the organism. Hence the greater part, and in the mature plant even the whole, of the food absorbed is devoted to this functional metabolism, so that only a certain fraction of the sugar which has disappeared from the solution is to be found in the resulting crop of fungi, in the form of various carbon compounds. The rest of the sugar has been burnt up to form carbonic acid and water; that is to say, it has been sacrificed to the physiological combustion, which in these, as in most plants, is indispensable for gaining an adequate amount of kinetic energy.

But, just as man is able to obtain driving power, not only from the combustion of wood and coal, but also from the explosion of gunpowder or dynamite, so there are certain of the lower plants which gain their whole kinetic energy by means of chemical transformations and decompositions, which go on without the participation of free oxygen. Although the careful consideration of such organisms is indispensable for any correct estimate of functional metabolism, yet we may, in the first instance, limit our attention to oxygen-respiration, i.e., to the functional metabolism of aerobic organisms.

In any case it is only after elaboration that food acquires its significance for the construction and working of the organism. With respect to the utilisation of the food, it is of no consequence whence it comes, or by what means the organism obtains it. Obvious as this consideration is, yet confusion between the elaboration of food in structural and functional metabolism on the one

hand, and the operations adapted to the acquisition and absorption of organic nutriment on the other, has led to a grave error, namely, to the assumption that a difference in principle exists between the metabolism of plants and that of animals. The simple reflection that an immense number of plants exist which are destitute of chlorophyll, might at once have taught that the function of the chlorophyll apparatus—the production of food from carbonic acid and water—only serves to provide nutriment for further elaboration, and to introduce it in a peculiar and highly characteristic manner into the organism. For the structural and functional metabolism of green plants, however, the sugar prepared in the plant's own factory has exactly the same significance as the sugar which a fungus obtains from outside. In like manner it makes no difference to the utilisation and importance of sugar in the metabolism of man, whether a sugar-baker obtains it from his own factory or another man has to buy it at second or third hand.

Again, only those plants can dispense with a supply of albuminous food substances which construct these bodies synthetically from simpler compounds. In all plants, however, in close analogy with the animal organism, albuminous substances not only serve as permanent constituents of the body, but are in part again disintegrated in metabolism. Yet this process does not as a rule result in an excretion of the nitrogenous products of decomposition, for the latter are usually at once re-employed for the regeneration of albuminous substances. This, however, cannot take place to a sufficient extent in cases where a mould fungus is provided with protein substances as its only food, so as to increase the transformation of albuminoids and at the same time to restrict their regeneration. Under these circumstances a large amount of ammonium carbonate is actually excreted, in other words, the same final product which also arises in the animal body, but which there at once undergoes condensation to form urea. By this latter process the injurious effect which would result from an accumulation of ammonium carbonate is avoided. In the case of fungi such an injurious accumulation does not usually occur under normal conditions of growth, while these plants also to some extent possess the power of guarding against its deleterious influence by neutralisation, owing to the fact that in the presence of alkaline compounds oxalic acid is produced in increased quantity.

It constantly happens that all those processes which do not form an essential part of the indispensable functional metabolism, are regulated in such a manner as to be wholly or partly brought to a standstill without impairing other functions. Any excessive accumulation of products always has this result, so that, for example, *the further formation of sugar or of protein bodies ceases, when these substances have collected in the cell up to a certain limited*

amount. On the other hand, the inevitable final products of the general functional metabolism must be continually formed, for it is upon this chemical process that the maintenance of vital activity depends, and these final products, in so far as they are not again made use of, must also be constantly secreted and removed, for their accumulation would render further activity impossible. In many aerobic organisms, for reasons already indicated, only the excretion of carbonic acid and water is in question. In the case of many fungi, and some other plants, we find, however, in addition to these substances, organic acids, and other non-volatile final products, which are secreted in great variety and amount, especially in the case of many aerobic and anaerobic fermentations.

In order to avoid an accumulation in the cell, the final products which are continually arising, as well as the food to be assimilated, must necessarily be soluble and capable of diosmosis. Hence the ejection of the undigested remains of food is usually impossible, though, where it is possible, we find it in plants as well as in animals, as, for example, in the *Myxomycetes*. Extracellular digestion, which is employed on an extensive scale even in the vegetable kingdom, is, broadly speaking, only a means by which substances are rendered available for absorption and elaboration by the living elements, but is no more an integral part of the actual functional metabolism than is the digestion in the stomach of animals. The same holds good with reference to respiratory movements, and to all those operations and adaptations which provide for the access of oxygen and the removal of carbonic acid. In plants, it is true, there are no special active respiratory movements, but in all the larger plants an extensive system of aëration serves to maintain, adequately, the gaseous interchange of the internal cells. A loose combination of oxygen, such as is found in the hæmoglobin of the blood, is not of general occurrence among plants, though present and efficient in certain chromogenic bacteria.

If we leave out of consideration all subsidiary and preliminary processes, there is no doubt that the true aerobic functional metabolism is the same, in principle, in plants and in animals; in fact even from a formal point of view, no difference exists, if, as is fitting, we select the lowest animal and vegetable beings for comparison.

In plants, which like animals perform a large amount of work, vigorous respiration also takes place; in specially active plants it may even be actually greater than in warm-blooded animals. For while in man, the carbonic acid produced in twenty-four hours amounts to about 1·2 per cent., in many mould-fungi it exceeds 6 per cent. of the weight of the body; in very active bacteria the consumption of oxygen, referred to the same standard, may reach an amount 200 times as great as in man.

Although such energetic physiological combustion involves a very considerable production of heat, yet in consequence of the extensive radiating surface, only a slight rise of temperature usually ensues. The functional metabolism, in fact, as in poecilothermic organisms, does not provide for the regulative maintenance of a definite body-temperature. At the same time the plant is adapted to accommodate itself to temperatures, ranging for example, from 2° to 40° C., and to bear such oscillations of the body-temperature without injury. Manifestly it is altogether expedient that, when the temperature rises, the activity of growth and respiration should be simultaneously accelerated. On the other hand, the fact that when the optimum temperature for growth has been exceeded, substances are burnt up by respiration, in an ever increasing degree, while the processes of growth and movement are retarded or altogether stopped, is a non-adaptive phenomenon, determined only by the absence of any regulative check.

As long as the external conditions remain constant, however, respiration is always regulated by the plant, and is in general increased, as the activity of the whole organism automatically rises. This happens, for example, when a plant begins to grow again, after completing the winter's rest, or when a traumatic reaction is called forth in consequence of injury. Thus, if we cut a potato in pieces, the production of carbonic acid gradually increases nine- or ten-fold, in the course of twenty-four hours, owing to the respiratory process, and then gradually diminishes again as the traumatic reaction passes off. Here the plant falls as it were into a state of fever, for simultaneously with respiration, the production of heat is very considerably augmented.

If we bear in mind that the essential office of functional metabolism, consists in providing, by means of chemical transformations, the necessary energy for vital action, we cannot be surprised that this end is not always attained in the same way. Apart from the fact that different carbon-compounds are consumed in respiration, and that not only carbonic acid, but in certain plants oxalic acid, acetic acid, citric acid, &c., arise as the final products of physiological combustion, there exist even aërobic organisms in which kinetic energy is no longer obtained by the oxidation of any carbon compounds whatever. Among these are the nitrobacteria, some of which oxidise ammonia to nitrous acid, while others oxidise nitrous to nitric acid; these remarkable organisms, with the help of the energy thus gained, are at the same time capable of constructing their organic food synthetically from carbonic acid. Again, in the respiratory process of the sulphur-bacteria, the sulphuretted hydrogen undergoes combustion, sulphur being first set free, and then oxidized to form sulphuric acid. Thus, in these organisms, sulphuric acid is secreted as

the final product of physiological combustion, while in the nitro-bacteria the same is the case with nitrous or with nitric acid. There can be no question that as time goes on, yet other specific peculiarities will be discovered. Thus it is conceivable that certain micro-organisms may gain their chemical kinetic energy by the oxidation of ferrous oxide, and others perhaps, by the oxidation of hydrogen or of the gaseous hydrocarbons.

Considered from the general point of view of energy, it is by no means necessary that physiological combustion should proceed along the same lines in all organisms. Indeed it was only in consequence of an unjustifiable generalisation from observations on the higher animal and vegetable organisms, that the belief arose that organic life is impossible without the agency of free oxygen, i.e., without oxygen-respiration. Just, however, as man is able to employ driving power, derived from such reactions as the explosion of gunpowder or dynamite in a space free from oxygen, so must it appear *à priori* possible, that organisms have been evolved on our earth, in adaptation to special conditions and necessities of life, which are able to live without making use of free oxygen.

As a matter of fact numerous anaërobic micro-organisms are now known. Their existence, indeed, was established as long ago as 1861, by the investigations of Pasteur, and it was only the deeply rooted belief in the absolute indispensability of oxygen-respiration which caused the majority of the learned to remain sceptical, or to endeavour to save the dogma of the necessity of oxygen by forced, and often, frivolous interpretations.

Much, it is true, remains to be explained as to the details of metabolism, both in aërobic and anaërobic organisms; meanwhile this at least is certain, that even in anaërobes, kinetic energy is gained by means of a great variety of chemical transformations. This fact is at once indicated by the various final products of functional metabolism, in one organism consisting chiefly of alcohol and carbonic acid, in another of butyric acid, lactic acid, or butyl-alcohol, or of other very various volatile and non-volatile compounds, which, however, often owe their origin in part to secondary processes; considered from the standpoint of energy it is not necessary that any gaseous products should arise, or even that oxygen atoms should be transposed, or carbon compounds disintegrated. It is true that the latter assumption holds good in all cases which have as yet been minutely studied, but it is quite conceivable that kinetic energy may be obtained by some other reaction (for example, by the reaction between potassium nitrate and sulphur).

The anaërobia are, however, of great importance in the economy of nature, for by their agency decomposition is carried on in the interior of the cadaver, and generally in places where the conditions

for oxygen-respiration are absent. Thus, in the case of facultative anaërobes, in proportion as oxygen becomes deficient, aërobic becomes replaced by anaërobic metabolism, while the obligatory anaërobes now for the first time begin their growth and multiplication. This is correlated with the fact, that oxygen, even at low tension, acts as a poison to them, and when air is so much compressed that the oxygen is twenty or thirty times as dense as in the atmosphere, all plants perish. All gradations of sensitiveness towards oxygen occur, from the most resistant organisms downwards, and there are even obligatory aërobes, such as the sulphur-bacteria, which can only exist when oxygen is of very low density.

Thus the different types are connected by intermediate links. For any facultative anaërobe it is possible to prepare a nutritive substratum on which it can flourish only when able to respire free oxygen. If, however, the oxygen be presented in sufficient dilution it does not hinder the growth of the anaërobic organism, which, under these circumstances, constantly draws the free oxygen into its functional metabolism, and thus gradually consumes it in considerable quantities. Further, a variable density of the free oxygen may be endured by the same organism in accordance with the cultural conditions under which it is placed. It has in fact been found possible, by means of special nutrition, to cultivate the strictly anaërobic *Bacillus* of symptomatic anthrax (*B. carbonis*) as an aërobe. The habits of the nitrogen-assimilating *Olostridium Pasteurianum* are also evidently modified by certain bacteria which usually live associated with it, for in such symbiosis *Olostridium* endures the free access of air, while in the isolated condition it can only live anaërobically.

Even in the case of typical aërobes, however, the withdrawal of oxygen does not bring the metabolic activity entirely to a standstill. At first carbonic acid is still given off, being derived from the intramolecular respiration, i.e., from chemico-physiological processes, which in most plants further result in the formation of alcohol and other products. This intramolecular respiration is thus a vital action which is of importance for the maintenance of life even in aërobes, but which in the latter does not suffice to maintain the whole working of the organism after oxygen has been withdrawn. In the anaërobes this capacity has been fully developed, but of course only comes into play when suitable nutrition is provided. For, when differently fed, even the facultative anaërobe can only grow if fully supplied with air, and behaves, when oxygen is withdrawn, just like a typically aërobic plant, for it then ceases to grow, and, sooner or later, perishes altogether. By means of an appropriate food-mixture, however, we can ensure that the facultative anaërobe continues its *growth and movements*, but only for a certain time and to a certain extent, thus appearing as a temporarily anaërobic organism. Such

an organism, for example, is yeast (*Saccharomyces cerevisiæ*), which, under the nutritive conditions hitherto tested, cannot live without an occasional supply of oxygen, whereas certain bacteria are capable of an unlimited anaërobic life.

It follows directly from what has been said, that among aërobes life is maintained for a limited time by the action of intramolecular respiration. For it is only when intramolecular respiration fails that the plant suffers in an atmosphere free from oxygen; the strictly aërobic mould-fungi keep alive much longer without oxygen, when their aërobic respiratory activity is intensified by providing them with sugar. Thus there is every gradation and transition between those organisms which require free oxygen, and those in which the anaërobic metabolic activity, which is exercised to a certain degree in all organisms, is so far developed and utilised that the functional metabolism suffices for a life without oxygen. Even in aërobes a number of partial functions are carried on for a certain time after the withdrawal of oxygen. Among these functions intramolecular respiration itself is included, as well as all the metabolic changes with which it is linked. There are also certain processes of growth and movement which are not at once brought to a standstill when oxygen is withdrawn. Thus we know that nuclear division, when it has once begun, still goes on in the absence of oxygen, and under the same conditions the tentacles of the insectivorous Sundew still carry out their movements when stimulated. The muscle of animals can also be caused to contract when deprived of oxygen.

The relations of the organism to its conditions of life can be demonstrated and understood, even though we do not possess any deeper insight into the causes and the exact processes of functional metabolism. We may also regard it as certain that functional metabolism is indispensable to vital activity, on which in its turn it depends and by which it is regulated, so that metabolism is at once extinguished when death ensues. Thus the realisation of functional metabolism ensures the continuity of metabolism in general, just as a blazing fire, by heating the wood, constantly creates and maintains the conditions necessary for the continuance of combustion.

It is also certain that functional metabolism runs its course within the living protoplasm, not merely on its surface or in particular portions of it, but in and between all its constituent parts, as must necessarily be the case in order that vital activity may be maintained. This can be seen at once from the fact that those movements in the protoplasm, or in any separate fragment of the protoplast, which are dependent on aërobic respiration, come to an end on the withdrawal of oxygen, even when the adjoining cells have access to oxygen and are in a state of full activity.

From the dependence of functional metabolism on vital activity it

directly follows that the consumption of the material to be elaborated, as well as the absorption of free oxygen by the organism, is regulated in accordance with its requirements. Consequently, when these requirements are fully satisfied, an increased supply of food material or of oxygen results in no essential acceleration of the functional metabolism. For this reason plants do not breathe any more vigorously in pure oxygen than in ordinary air, for even in the latter much more oxygen penetrates into the cell than is consumed in normal respiration.

If, however, the supply is not sufficient to fully satisfy the demand, then the functional metabolism, and with it the whole activity of the organism, is unavoidably reduced, just as a fire can no longer burn properly when insufficiently supplied with fuel or with oxygen. Most plants, however, can completely meet their demand for oxygen in an atmosphere in which the proportion of oxygen (at ordinary atmospheric pressure) is reduced to 5—8 per cent., so that on the highest mountains vegetable organisms find a more than sufficient density of oxygen. If its density be still further diminished, then, after a transient disturbance, the respiration and the total activity of the plant are depressed, so that in an atmosphere containing only 2—4 per cent. of oxygen the plant, though it survives, breathes and works in a diminished degree.

Since functional metabolism depends on the vital activities, a satisfactory causal explanation of the former will only be possible after we have gained a sufficient insight into the latter. In general, however, we may say that the same processes which effect intramolecular respiration also develop those affinities, by means of which free oxygen, when supplied, is drawn into metabolism. For intramolecular respiration is at once stopped on access of oxygen, and after the withdrawal of oxygen is immediately resumed. In much the same way as the development of the spontaneously inflammable phosphuretted hydrogen brings about the fixation of a certain amount of oxygen, may physiological combustion be determined and regulated through the continual formation of a single autoxydable body. In respiration, however, we have evidently to do with complicated reactions and reciprocal changes, which come into play between the constituent parts of the protoplasmic body. And further, the respiratory process must take a somewhat different form when, instead of a carbon compound, ammonia or ammonium nitrite, sulphuretted hydrogen or sulphur, forms the material for physiological combustion.

We may be sure, however, that in the plant passive oxygen is drawn into metabolism, and that oxygen is not brought into the *active state* in order to accomplish physiological oxidation. For we *can prove with complete certainty* that at no time does any such

process of oxidation occur in the interior of vitally active protoplasm (including the nucleus), as is brought about by even the feeblest form of active oxygen (hydrogen peroxide). This still holds good even if the reactions of active oxygen are obtained in the expressed sap, i.e., after mixing bodies which in the plant are separate. If, however, active oxygen ever plays any part at all, it is at most to be regarded as one only of the means of which the organism avails itself, and not as revealing the true and essential cause of functional metabolism.

Side by side with the general process of functional metabolism, many other chemical operations must necessarily come into play, in order to provide the various compounds which are formed in the organism, in order to build up its tissues, or otherwise. Although these processes are not of necessity in continuous action, it is difficult to separate them from the general functional metabolism. We are placed with regard to the plant, somewhat in the position of a man who, while he can control the raw material introduced into a factory and the finished products turned out from it, is not permitted to inspect the internal working. Unless the observer has a knowledge of this from other sources, it is simply impossible for him to say what is the nature of all the manifold operations carried on in a chemical factory, whether simultaneously or successively, jointly or separately. At the same time the observer may be quite aware that all work in the factory is impossible if the fire be not burning under the boiler, or if the driving power in general be not available, and he may also know that the gaseous products of combustion, the ashes and the slag, must be got rid of, simply in order to make room for the work to go on. In the factory, however, just as in the plant, the general driving power is not always utilised for the same purposes or with equal efficiency. Indeed, when the steam-engine is at work but the rest of the machinery is out of gear, the whole driving power is wasted. No less is it true, in the case of the plant, that the relation between the available kinetic energy and its utilisation for various purposes, or in other words the economic coefficient, may vary within very wide limits according to the stage of development and the external conditions.

BAKERIAN LECTURE.—“Further Experiments on the Action exerted by certain Metals and other Bodies on a Photographic Plate.” By W. J. RUSSELL, Ph.D., V.P.R.S. Received February 10,—Read March 24, 1898.

In a paper read before this Society in June last* it was stated that certain metals, alloys, and other substances such as picture copal, printing ink, straw board, &c., were able to act even at a distance on a sensitive photographic plate, producing effects similar in appearance and developed in the same way as plates which had been acted on by ordinary light. At that time sufficient experimental evidence had not been obtained to determine the nature of this action, or even to clearly indicate its general character, whether in fact the action arose from vapour given off by the active body, or whether phosphorescence was produced which acted on the plate. That bodies so slightly volatile as zinc, aluminium, nickel, &c., should be able to give off at ordinary temperatures in a few days sufficient vapour to act strongly on a photographic plate, and that such vapour should be able to pass rapidly through media, such as gelatin, celluloid, collodion, &c., seemed difficult to realise, although many of the earlier experiments appeared to indicate that this was the kind of action which took place. Later experiments confirm the view that a vapour is given off, which is the cause of the action on the plate.

Certain organic bodies, as well as metals, have been shown to act on the photographic plate, and in endeavouring to ascertain the nature of this action experiments with organic bodies were first undertaken, as the results which they yield are more easily and rapidly obtained than those with the metals, and if their mode of action was determined it would probably throw light on the action exerted by the metals. In the former communication it was stated that printing ink and copal varnish are active substances, both when in direct contact with a photographic plate and when at a distance from it. Further it was found that the action which they exerted was able to pass through different media. Although printing inks and copal varnishes may vary considerably in composition, the main constituents are constant, hence it was easy to determine that boiled oil and turpentine were the bodies to which they owed their activity, and that these bodies separately behaved in the same way as did printing ink and copal varnish. Boiled oil—that is, linseed oil which has been heated with oxide of lead—is an active substance, and most of the following experiments have been made

* ‘Roy. Soc. Proc.’ vol. 61, p. 424.

with the pale drying oil which is prepared by Messrs. Winsor and Newton. Pure turpentine is also a very active substance, and, owing to its volatility, in many experiments very useful. These bodies can be used either as liquids in small dishes or by saturating Bristol board or other neutral and porous bodies, such as ignited pumice stone, &c., with them. In the case of the drying oil it can be painted on glass or cardboard and allowed to harden.

The experiments described in the former communication have been repeated under the same and under slightly different conditions and in another laboratory; the results obtained with one exception confirm the previous statements. Glass, selenite, mica, even in very thin layers, are absolutely opaque to the action, whereas gelatin, celluloid, collodion, guttapercha tissue, tracing paper, parchment, and paper are more or less transparent. Linseed oil and turpentine may fairly be taken as typical examples of active organic bodies. This property of acting on the photographic plate is far from belonging to all volatile organic bodies; for instance, although vegetable oils have the power of acting, mineral oils, so far as they have been tried, have not the power. Benzene, carbon disulphide, chloroform, &c., also are without this power of acting on the photographic plate; but the question of what substances are and what are not active will be dealt with on another occasion; at present it is simply to consider the conditions under which linseed oil and turpentine act on the photographic plate. The picture copal varnish which was much used in the former experiments obviously owes its activity to the turpentine and the oil it contains. Warm the varnish for some length of time, these bodies are driven off and an inactive gum remains. This experiment obviously suggests a vapour as the cause of the action; at the same time would such a vapour pass through bodies such as gelatin, celluloid, &c.? With regard, for instance, to the thin-sheet gelatin, it appears to offer but very slight obstruction to the action of these organic bodies; if the thickness of the gelatin be increased still the action takes place, only the time of exposure has to be considerably longer.* Another striking fact with regard to this emanation from these active bodies is that it gives an accurate picture of the surface from which it has come. A hard copal surface on glass will give a picture showing every brush mark, unevenness, and scratch on the surface, and if the action take place through a thin sheet of gelatin, or even as many as six or more sheets, still the picture of the scratches is distinct. The following experiments are apparently strong evidence that the action on the photographic plate is due to the vapour given off from these organic bodies.

A piece of Bristol board saturated with drying oil, or a piece of

* The thin gelatin is 0.02 mm. and the thick 0.16 mm. in thickness.

glass painted with it or with picture copal, is placed on the bottom of an ordinary plate box and a photographic plate larger than the active body is suspended above it with the film upwards; light is excluded from the box, and the arrangement is left for, say, a fortnight, then the plate when developed will be found to have been acted on irregularly round its edge, at some parts considerably more than at others, but everywhere shading off and evidently in the way which would occur if a vapour had rolled round the edge. Another experiment which showed this kind of action very satisfactorily was carried out as follows:—A circular piece of the Bristol board was saturated with drying oil, and at a little distance above it a smaller circle of mica, which is perfectly opaque to the action, was placed, and again above this was another piece of mica with a circular hole smaller than the circular mica plate, and then the photographic plate was placed above. By this arrangement no direct action could take place between the drying oil and the sensitive plate, but a vapour could work its way between the mica plates and thus reach the photographic plate; and this it did, for after an exposure of three days, on developing the plate there was a dark ring formed shading off towards the centre. Another and very simple experiment illustrating this same point is to place a small circular glass dish, with some drying oil in it, in the middle of a photographic plate, and leave it there for a week. On developing the plate it will be found that no action has taken place where the dish stood, but that immediately beyond the outside of the dish much action has occurred, and that the darkening gradually fades away. There is still another way in which the action of these organic bodies has been tested, and that is by transferring the active power of these bodies to a neutral substance. If vapour be the immediate cause of the darkening of the photographic plate, then it would be possible, if a piece of Bristol board were suspended above drying oil, for instance, for the inactive board to take up those vapours and become photographically active. This was found to take place. Bristol board of good quality is a very useful substance in all these experiments, both as a screen and as an absorbent. It is in itself an inactive body, and may be heated in a water bath before using, to prevent the accidental presence of any substance which might act on the plate. If a piece of the Bristol board be suspended above drying oil, in the liquid or solid state, or turpentine or picture copal for two or three days, or even less, it becomes strongly active, and when placed in contact with a photographic plate quickly darkens it. This action of the Bristol board is well shown if a pattern be stamped upon it, which is easily done by pressing against it a piece of white net (black net must not be used as it is slightly active), then the charged Bristol board will *give an unmistakable picture*. If turpentine be the active sub-

stance used to charge the Bristol board the exposure need only be for a few hours, but if after this charging it be exposed to the air for a day or two, its activity will be found to have gone. There is obviously no visible indication of this activity of the Bristol board, and consequently if a device be cut out on a screen which is placed in front of a sheet of cardboard, or any inactive paper, and it is exposed to turpentine or to oil, or if the vapour of these bodies be in any other way brought in contact with the paper, a dark picture of such device, which is not visible, may be produced. Some unexpected and curious cases of ghostly pictures thus formed have been met with, they are, however, all produced in this way, and need not be described now. The above experiments have been made at ordinary temperatures, but if the temperature be increased the activity of these organic bodies is also greatly increased. High temperatures cannot, of course, be used, but a temperature of 55°C . does not appear to alter the photographic plate. With drying oil—which is one of the most active substances that has been used—it is easy to obtain a picture in thirty minutes at the above temperature. The interesting pictures which in the former communication it was shown could be produced simply by laying a piece of dry wood or the section of the branch of a tree on a plate are produced by the volatile matter contained in the wood. These pictures appear at first sight very accurate and complete, but this is not really so, for some parts of the structure of the wood are not shown and other parts are too strongly developed, depending on the amount of volatile substance present in the different parts. It is, however, very remarkable that so small a quantity of the volatile body as exists in a piece of dry wood should be able to produce a picture; the activity of the wood is increased by the presence of moisture. This property of acting on the photographic plate, possessed by the linseed oil, belongs apparently to the vapour and not to the oil itself, for if a sheet of thin gelatin be placed on a photographic plate, and on it a thick glass ring nearly filled with oil, and over the top of the ring another sheet of gelatin, not in contact with the oil, and another sensitive plate, it will be found that after a week's exposure no action has taken place below the oil, but that a large amount has occurred above it where the vapour has penetrated the upper sheet of gelatin. A similar result is also obtained by simply floating a piece of the thin gelatin on a dish of oil and placing a sensitive plate above. At the sides where the vapour can form and get away there is action on the plate; in the centre there is none.

In addition to glass, mica, and bodies of that kind, the action does not take place through a layer, except it be very thin, of either gum arabic or of paraffin. If a piece of Bristol board or a glass plate has hardened drying oil on it, and be painted over with a strong solution of gum arabic which is allowed to dry, then the delicate

cracking which occurs can be very well shown on a photographic plate.

Pure water does not act on the plate, neither does pure alcohol or pure ether, but the ordinary commercial specimens of the last two bodies do, and often to a considerable extent. Alcohol which produces a dark picture will, after digestion with lime and careful distillation, be entirely inactive, and ether after careful purification also becomes inactive. Moisture, if present, does not affect this result; thus the presence of certain impurities, and they appear to be some of the most common ones, can be readily photographed, and approximately their amount determined by the darkness of the picture formed, so that by this means can be determined whether, for instance, a purification process is working well and whether it has completely done its work. The pictures are easily obtained by placing some of the liquid to be tested in a small glass dish, and a sensitive plate above it. Obviously it is only certain impurities which will be indicated in this way, but the reaction is otherwise of considerable importance, for it gives a simple method of determining what bodies soluble in these liquids, are capable of acting on a sensitive plate. This matter will be treated of in a separate communication.

That the vapour given off by these organic bodies is the immediate cause of the action on the sensitive plate the above experiments seem to show. At the same time it is remarkable that such a vapour should readily pass through media such as gelatin, celluloid, &c., and not by mere absorption, but in such a way as to produce a picture of the surface from which it emanated.

Passing on to the action which certain metals exert on a photographic plate, results have been obtained which are strikingly similar to those just described. Substitute a piece of polished zinc for a piece of Bristol board saturated with linseed oil, and similar effects are produced on a photographic plate; the time of exposure must, however, be longer. Although both magnesium and cadmium are slightly more active than zinc, this last metal is the most convenient one to experiment with, and has been used in most of the following experiments. In addition to the above three metals, nickel, aluminium, lead, and bismuth have the same property, but not so strongly developed, of acting, both when in contact and when at a distance, on a photographic plate. Cobalt, tin, and antimony can also act in the same way, but their action is considerably feebler, and undoubtedly other metals can act in the same way, but require much longer exposure. Mercury, which in the former paper was stated to be the most active of all the metals which had been tried, is now found to be quite inactive; the metal used in the former experiments was impure. This matter will be explained further on.

As so strong a similarity exists between the effects produced by

the above-mentioned organic bodies and the metals, the question which naturally presents itself is, do they also give off a vapour which directly or indirectly acts on the photographic plate? The following experiments show that such an action does probably occur. Zinc which has been long exposed to the air is inactive. An exposure out of doors for only three or four days diminishes very considerably its activity, but covered up in doors after three weeks it will still give a tolerably dark picture. If it has a bright but perfectly smooth surface it is active, but not strongly so; rub the zinc with coarse sand or emery paper, and it is then obtained in its state of greatest activity; the same applies to all the metals. If, when in this condition, any of the active metals be placed in contact with a photographic plate, a beautifully sharp picture of the scratched surface is obtained. The great increase of the fresh metallic surface produced by the rubbing may account for the increase of activity which the scratching produces. If the zinc plate be raised only slightly above the photographic plate, a sharp picture of the scratches is still obtained, and of course as the distance is increased, so is the indistinctness of the picture increased, and at last it fades into a general cloudiness, and in this form the zinc plate can easily be made to act through the distance of an inch or more.

This action of the metals passes through the same media as do the vapours from the organic bodies, and clear pictures can be obtained through sheets of thin gelatin, &c.; in fact what has already been said with regard to the transmission of the activity of the organic bodies applies to the metals; gelatin, both thin and thick, allows the action of the metal to pass through it; celluloid and collodion do the same, and so does gold-beaters skin and tracing paper. Reasoning, then, from this strong analogy between the action of the organic and the metallic bodies, it must be assumed that the above-mentioned metals from a clean surface and at ordinary temperatures give off vapour, and this vapour apparently acts when under the same circumstances in a like manner to the vapour given off by the drying oil. It gives a clear picture of the metal surface from which it arose, and it can permeate the same media as the organic vapours. The remarkably clear pictures of, for instance, a zinc surface, which can be produced through a sheet, or even several sheets, of the thin gelatin proves that the action is not one of mere absorption.

To gain further knowledge on this point and test the porosity of these different media, the power of hydrogen to diffuse through them was tried by cementing specimens of the different substances on glass tubes, which were filled with hydrogen and placed over water. The action is somewhat remarkable, but requires further confirmation. With the thin gelatin ordinary diffusion does not occur, and a hardly, if any, perceptible rise of the water in the tube

occurs on starting the experiment; but on standing for some length of time, two or three days, the water begins to rise, and after a week or more it will stand at a height of four or more inches above the level in the vessel, and there it remains at least for a month or more. With the thick gelatin there is no evidence of any diffusion occurring. Celluloid acts much in the same way as the thin gelatin, a column of water gradually rises and remains there. The action of the guttapercha tissue is to absorb the hydrogen; the diffusion tube completely fills up with water and remains full without showing any tendency to fall for a couple of months, and then the experiment was stopped. With tracing paper diffusion occurs in the ordinary manner, and the same happened in the single experiment tried with gold-beater's skin. That the rise of the water in the diffusion tubes is not owing to a mere absorption of the hydrogen by the gelatin or guttapercha has been proved by placing a considerable quantity of these bodies in a tube sealed up at one end, filled with hydrogen and inverted over water; after several weeks no rise of the water in the tube occurred. The above experiments have been repeated with the same results, but further trials are being made. Possibly the metallic vapour is in a still finer molecular state than ordinary hydrogen, and thus is able easily to permeate a medium which hydrogen can only slowly get through, and air cannot get through. At all events, this may be looked upon for the moment as a working hypothesis.

That the action of the metals like that of the organic bodies is due to a vapour can be demonstrated by experiments exactly similar to those already described. For instance, if the thin mica plates be arranged above a zinc plate, in the way already mentioned, so as to cut off all direct action between the zinc and sensitive plate, a ring of action is produced which can only be accounted for by supposing a vapour present, which has worked its way between the sheets of mica and thus gained access to the photographic plate. Again, a piece of Bristol board can be made active by contact with, or mere proximity to, a piece of polished zinc. A striking instance of this arose in the following way: a piece of perforated zinc had lain on the bottom of an ordinary plate box for a considerable length of time, probably about two months; the zinc was then taken away, and a sensitive plate dropped into its place. On developing this plate, a picture of the perforated zinc was obtained. Other experiments of a similar kind have been made. If the Bristol board be not in direct contact with the zinc; if a screen, with holes cut out in it, be interposed, it will be found that the Bristol board where exposed to the direct action of the zinc will become active, and will give an exact picture of the holes or whatever design it may be *which has been cut out on the screen*. To produce this effect the

cardboard has to be exposed to the zinc for fully six weeks. This changing of the Bristol board does not take place satisfactorily above ordinary temperatures. With other metals than zinc, these changing effects have not as yet been obtained.

Another experiment, which illustrates the way in which the metals can act, is to take a piece of ordinary perforated zinc, polish one side, and lay this polished side against a plate of plain glass in a printing frame, then place the photographic plate against the dull side of the perforated zinc, and leave it in the dark for three or four days; then, on developing the plate, a reversed picture is obtained, that is, the holes in the zinc will be represented by dark spaces, and the zinc itself by light ones. If the holes in the perforated zinc are large, they are represented by shaded circles, so that these pictures are produced by the vapour emitted by the polished zinc which has crept into the open spaces and thus gained access to the photographic plate. It has already been shown that the action exerted by zinc passes more readily down a glass than down a paper tube of the same size; this has been strikingly confirmed by taking two pieces of glass tubing 1 inch long $\frac{3}{4}$ inch in diameter; inside one a single coil of inactive paper was placed, and both tubes stood on a sheet of polished zinc, and a photographic plate rested on the top of them. They were then left for a week, and on developing the plate, a black patch appeared above the tube without the paper, and no action was visible above the one with the paper. Without removing the paper, it was painted over with melted paraffin, and again a photographic plate put on the top of the two tubes; now two circular dark patches were produced of equal intensity.

If the activity of the zinc depends on a vapour which it emits, it seemed possible that it could be carried along by a stream of air. In order to try whether this was the case, a tube a foot long was packed with zinc turnings which had been amalgamated, and a stream of pure air sent through it. The end of the tube was fixed into the side of a dark box and a sensitive plate with a screen upon it suspended above it, thus no direct action could be exerted by the amalgamated zinc on the plate. The experiment was continued for four days, then, on developing the plate, a picture of the screen above where the tube entered the box was obtained, but at the other end of the plate there was no action.

The presence of mercury in this experiment was unsatisfactory, and might account for the result obtained, therefore an exactly similar experiment was made, and zinc turnings alone were used and a plate without a screen. The experiment was carried on for a week, and it was then found that a black patch had been produced immediately above the end of the tube. To be sure that this darkening did not arise from the action of the air, the whole of the zinc was

removed from the tube and again the air sent through it for a week, the sensitive plate showed no signs of any action having taken place.

To try whether any accumulation of the vapour, and hence an increase of action could be brought about by an increase of zinc surface, two small circular glass dishes were taken, about $1\frac{1}{2}$ inches in diameter and $\frac{1}{2}$ inch in depth; into one a single disc of bright zinc foil was placed, and in the other twenty similar discs, then on the top of both vessels a photographic plate was placed. The single disc was raised on a piece of glass, so both end discs were at the same distance from the plate. The discs were a little smaller than the glass vessels, and, owing to their not being quite flat, there was a space between each one. In two experiments there was no marked difference between the density of the pictures produced, the single disc produced as much effect as the twenty. A more direct way for the passing off of the vapour was then made by cutting a circular hole $\frac{3}{8}$ inch in diameter through the centre of all the zinc discs, and now a very black central spot was formed by the twenty zincs, and of course there was a white spot with the single disc, so that the vapour accumulated to a considerable extent in this central space.

It has already been mentioned that the statement in the former paper that mercury was the most active of the metals is incorrect. The error arose from not suspecting that a trace of any impurity would affect the activity of the mercury, and, consequently, not taking special precautions to insure its perfect purity. On repeating the former experiments with another sample of mercury it was found that no action occurred, which seemed very remarkable; moisture was added, the temperature was increased, but still no action took place; the addition of a little zinc to the mercury was then tried, and it was found that this made the mercury excessively active. The presence of a very small quantity of zinc is able to effect this change, certainly less than 1/300th per cent. It is very remarkable that so small an amount of the metal can cause so strong an action on the photographic plate, for the exposure to the vapour given off by such an amalgam need not, even at ordinary temperature, be longer than two to three days. If other active metals are dissolved in pure mercury they act in the same way, at all events, this applies to magnesium and to lead. If silver, on the contrary, be added it does not render mercury active, nor does sodium. This action of mercury, which contains zinc or lead, the most common impurities, is so readily recognised that it becomes a valuable test for its purity, and a very interesting means of following the effect produced by any purifying process. A specimen of mercury containing not more than 1/300th per cent. of zinc gave a *very dark picture*; this mercury was then treated first with sulph-

uric acid and afterwards, for three days, with nitric acid, and the picture it then gave was very faint, and on repeating this purifying process no picture at all was produced. Again, a sample of mercury containing zinc was carefully distilled. The distillate gave a very faint but very distinct picture. Another sample of distilled mercury also gave a faint picture.

Temperature, as might be expected, affects greatly this activity of the metals; at 4° or 5° C. zinc has but little action on a photographic plate. Most of the foregoing experiments have been made at about 17° or 18° C., and some, as specially noted, at 55° C. The Ilford special rapid plates have been used.

It appears, then, from the above experiments that certain metals have the property of giving off, even at ordinary temperatures, vapour which affects a sensitive photographic plate, that this vapour can be carried along by a current of air, and that it has the power of passing through thin sheets of such bodies as gelatin, celluloid, collodion, &c., in fact, so transparent are these bodies to the vapour that, even after it has passed through them, it is able to produce clear pictures of the surface of the metal from which it came. That much remains to discover with regard to this action of the metals is obvious, the most active metals are not the most volatile. Nickel is very active, cobalt only very slightly so, copper and iron are practically inactive. I hope before long to be able to bring before the Society further developments of these curious actions, both of metals and organic bodies.

The foregoing experiments have been made in the Davy-Faraday laboratory, and I beg to thank the managers of the Royal Institution for having allowed me to work in their laboratory. My thanks are also due to my assistant, Mr. Block, for the very careful and intelligent way in which he has aided me with the experiments.

March 24, 1898.—Additional experiments have been made with active organic substances, in order to determine to what class of bodies they belong. As already stated, linseed oil and turpentine were the two substances first found to be very active organic bodies, and following out these results it has been proved that vegetable oils in general have more or less this property of acting on a photographic plate. Linseed oil, for instance, is very active, olive oil only very slightly so. Even samples of "pure" linseed oil obtained from the different artists' colourmen vary considerably in the amount of action which they exert. The boiled, or drying linseed oil, has, at ordinary temperatures, the same activity as the ordinary oil, but under other conditions it appears there is some difference in their action. Another class of bodies, also called oils, namely the essential oils, have been found to be very active substances.

112 *Action of certain Metals, &c., on a Photographic Plate.*

Samples of commercial specimens of the following essential oils have been tried:—Peppermint, lemons, pine, juniper, bergamot, winter green, lavender, cloves, eucalyptus, cajeput, and cedrat, and were all found to be active, and not only when used by themselves, but also when dissolved in a large amount of pure alcohol. It is well known that the active substances in the essential oils are bodies known as terpenes, and I have to thank Dr. Tilden for supplying me with pure specimens of these bodies; they are all of them remarkably active. Paraldehyde and benzaldehyde are also very active bodies. Ordinary aldehyde and formaldehyde, as they have been at present applied, are only slightly active. Guaiacum, both when in powder and in alcoholic solution, is active, and so are powdered cinnamon, sweet spirits of nitre, and eau de cologne. Brandy is slightly so. Now the important property belonging to all these bodies is their reducing or oxygen-absorbing power, hence the conclusion that it is this property which enables them to act on the photographic plate. The mineral oils, purified petroleum spirit, alcohol, ether, the esters, such as ethyl acetate, benzene, nitrobenzene, are all, when pure, unable to produce any effect on a sensitive plate, and even oxidised bodies nearly related to the terpenes, such as terpinol and camphor, are not active, neither is thymol. Terebene is an exceedingly active body. The difference of activity of the ordinary oils seems to follow their oxygen-absorbing power, at all events it is so with regard to linseed and olive oils, for the former is the most active of the oils, and 1 gram of it is said to be capable of absorbing 186 c.c. of oxygen, while the same weight of olive oil can absorb only 8.2 c.c. It is also interesting to note that, at least with some of these active bodies, results can be obtained which correspond to what photographers term solarisation or reversal, the action, when modified, giving a black picture, when carried to its full extent a white one.

It has already been stated that a mere trace of zinc makes mercury active, but it is certainly equally curious and unexpected that a trace of zinc should make alcohol active. It is only necessary to take pure alcohol and place in it some strips of bright zinc foil, leaving the metal in for three or four days. It will then be found that the alcohol can act strongly on a photographic plate. The same happens with ether and with ethylacetate, but not with benzene. In addition to zinc, cadmium, magnesium, aluminium, and fusible metal can act in the same way, whereas lead, nickel, tin, silver, sodium, and, as far as experiments have gone, all the inactive metals, have no such power. This reaction is still being investigated, but certain it is that careful filtration does not remove this activity from alcohol, nor does distillation entirely destroy it.

"On Contact Electricity of Metals." By J. ERSKINE-MURRAY, D.Sc., F.R.S.E., Heriot-Watt College. Communicated by Lord Kelvin, G.C.V.O., F.R.S. Received August 4, 1897, —Read November 18, 1898.

- I. Introductory.
- II. Method of Experiment.
- III. Effects of Different Methods of Cleansing the Metallic Surfaces.
- IV. Thin Solid Films of Oxides, Iodides, &c.
- V. Atmospheric or Time Effects.
- VI. Very Thin Liquid Surface Films on Metals.
- VII. Thick Liquid Films on Metals.
- VIII. Films formed from Gases.
- IX. Temperature Variations.
- X. Elimination of Metal-Air Potentials by Solid Non-conducting Films on the Metallic Surfaces.

I. Introductory.

§ 1. The experimental investigation described in this communication had as its primary object the elucidation and measurement of the variations of Volta contact electricity of a pair of conductors, due to changes in the state of *that portion of the surface of each conductor which was separated from the other conductor by an insulating medium.*

§ 2. The discovery of contact electricity of dry metals in air by Volta at the beginning of the present century, extended a quarter of a century later by Pfaff to dry varnished metals in other gases, has been confirmed by many subsequent experimenters. The reality of the electrostatic force in air near an interface between copper and zinc, inferred as an obvious consequence from it by Lord Kelvin, was experimentally demonstrated by him in 1861*. In the next twenty years many investigations were made, the more important being those of Hankel, Gerland, Clifton, Ayrton and Perry, and von Zahn. In 1881 a paper of great importance was published by M. Pellat,† and as the present communication is, in some respects, only an amplification and extension of his work, it may be of advantage to give a short *résumé* of it before going further.

M. Pellat's most important results appear to be (1) his demonstration of the influence of the physical condition of the metallic surfaces on their Volta-potential; thus he found that a sharply scratched plate is positive to a more smoothly polished one of the same metal, the metals being washed with alcohol after polishing in both cases and allowed to dry before the measurement of their potential; (2)

* 'Electrostatics and Magnetism,' § 400 *et seq.*

† 'Ann. Chim. Phys.,' 1881.

his measurements of the temporary variations of potential due to change of temperature of a copper, iron, or zinc plate; and (3) his experiments which prove the smallness of the changes produced in the potential-difference of copper and zinc by varying either the pressure or the nature of the gas surrounding them.

§ 2a. I shall now give a short summary of the results detailed in the present paper:—

(a) Metals covered with non-conducting solid films of wax or glass, except at their point of contact, give nearly the same potential as the bare metals in air. The substitution of wax for air next the metal only causes a small change which may be in the same direction and of approximately equal amounts for metals whose potentials in air are very different; e.g., the Volta-potential-difference between zinc and copper when both are coated with solid paraffin wax is very nearly the same as that between bare zinc and copper. In this connection I may mention that I have measured the potential of sodium coated with wax and glass, and find it to be about 3.56 volts positive to a standard gold plate.

(b) A metal cleaned by careful polishing and scratching with emery-paper or glass is less positive when its surface is in a sharply scratched condition than when smoothed or burnished, the difference frequently amounting to 0.2 or 0.3 of a volt.

This result is not in opposition to that of M. Pellat mentioned above (§ 2 (1)), for the conditions were different, as his plates were washed with alcohol after polishing, while mine were not (see Chap. III, and also §§ 39 *et seq.*).

(c) The temperature variations, between 15° C. and 60° C., of the Volta-potential of many metals have been determined, both for clean dry metals in air and for metals coated with liquid or non-conducting solid films; and it has been found that they are of considerable magnitude in both cases. The curves representing the variation of potential with temperature appear as if they should meet at a point below -200° C., at a potential about 0.4 volt positive to a standard gold plate at 16° C.; this suggests that Volta-potential-differences may possibly vanish at a very low temperature (see Chap. IX).

(d) A liquid film, even if of extreme thinness, may cause a considerable change in the potential of a dry polished plate, which continues permanent for many hours and even days after the disappearance of the film.

Two films of the same liquid opposed to one another on the surfaces of two plates of different metals do not usually give zero potential-difference, as solid conducting films of one material would do, but give nearly the same potential-difference as the dry metals on which they lie (see Chaps. VI and VII).

(e) A very thin film of oxide on a metal produces only a very

small change in the potential, and every increase in thickness of the film is attended by a further change in potential until a limiting value is reached, which is that of a mass of the oxide (see Chap. IV).

(f) Exposure to the atmosphere at ordinary temperatures does not, as a rule, produce any rapid change in Volta-potential, especially if the air be comparatively dry and free from dust. The ultimate change is usually in the negative direction (see Chap. V).

(g) I have extended Lord Kelvin's experiments on the effect of temporary immersion of a metal in a gas to the cases of copper, zinc, tin, and silver in oxygen, and find that copper, zinc, and silver become temporarily positive, while tin becomes negative in consequence of this treatment (see Chap. VIII).

The research, suggested by Lord Kelvin, was carried out in the Natural Philosophy Laboratory of Glasgow University during the Sessions 1893-94-95, and during 1895-96 in the Cavendish Laboratory of Cambridge University. My thanks are due to Lord Kelvin for many suggestions and much valuable advice, both in regard to experiments and to the discussion of results, and to Professor J. J. Thomson for similar kindnesses during my work in the Cavendish Laboratory. A small portion of this investigation, on the effect of Röntgen x -rays on the contact electricity of metals, was published in the 'Proceedings of the Royal Society' for March, 1896.

I have also to thank Professor James Holm, M.A.* and Mr. George E. Allan, B.Sc., for the part they took in the earlier portion of the work.

II. *Method of Experiment.*

§ 3. The measurement of the natural potential-difference between any pair of conductors was usually made, in air, by the null method described very briefly by Lord Kelvin in the Report of the British Association for 1880, p. 494. To make clear the exact circumstances in which the potentials were measured it will be advantageous to describe the apparatus and general method of experiment in detail.

§ 4. A circular disc of one of the metals, usually about 9 cm. in diameter and 0.2 cm. thickness, is insulated and in permanent connection with the insulated pair of quadrants of an electrometer. A similar plate of the other metal placed parallel to the first, at a distance of a few millimetres from it, is uninsulated and in connection with the uninsulated pair of quadrants. While in this position they are temporarily connected with one another through a simple form of potential-divider in which a slope of potential is maintained by a

* When this was written last August, my friend and former fellow-worker was Professor of Applied Mathematics in the South African College, Cape Town. He died in October.

Daniell cell in the direction opposite to that given by the experimental plates. The temporary connection is now broken and the plates are separated. In doing so the capacity of the condenser is reduced, hence if there be any electric charge on them it will be indicated by a further deflection of the electrometer. This operation is repeated with different values of the counterpotential until separation of the plates produces no change in the deflection of the electrometer. Since it has annulled the charge, the counterpotential must now be equal and opposite to the natural potential-difference.

§ 5. During most of the experiments a piece of apparatus made by Lord Kelvin in or about 1861 was used. In it the plates were surrounded by a cylindrical zinc case. This was made in two parts for convenience in manipulation, the lower fixed to a heavy cast-iron base plate, while the upper stood upon small brackets attached to the top edge of the lower part, its position being regulated by a hole-slot-and-plane arrangement. Openings in the sides of the upper part facilitated the adjustment of the experimental plates, but these were closed by a sliding cover during a measurement of potential.

The lower plate was supported by an insulating glass stem which was kept dry by means of pumice and sulphuric acid. On the top of this stem was a brass cap screwed to fit the sockets which were soldered to the backs of the plates, and from the brass cap a stiff wire passed out through a hole in the case and formed the connection between the insulated plate and the electrometer.

A piece of platinum foil was soldered round a part of this wire to give a clean contact for the temporary connection between the plates through the potential-divider, and the wire from the divider had likewise a platinum end piece.

The upper plate hung, by a ring at the centre of its back, on a hook at the lower end of a vertical metal rod, which could be drawn up so as to increase the distance between the plates. A small disc fixed on the rod at right angles to it just above the hook had three screws passed through it and pressed against the plate, holding it firmly in the hook; by this means the upper plate can be set parallel to the lower one in a very short time. This apparatus, which is the same as that used by Lord Kelvin in his experiments many years ago, was found to be very convenient, the arrangements enabling us to take out and replace a plate very rapidly, a matter of great importance when observing temporary variations, while at the same time both plates were firmly attached to their supports when in position.

§ 6. The divider, whose total resistance was about 2400 ohms, was made to divide the potential-difference between its outer

terminals into 100 equal parts. The potential applied was usually that given by a Daniell cell of Lord Kelvin's gravity type, and was tested frequently by comparison with a standard Clark cell, or by means of a standard resistance and a current balance.

§ 7. The permanent connections were as follows :—(1) The lower plate to the insulated pair of quadrants of the electrometer; (2) the terminals of the Daniell cell to the ends of the divider (there was a reversing plug in this circuit); (3) one end of the divider to the upper plate and uninsulated quadrants of the electrometer.

The temporary connection (§ 5) was from the sliding contact piece of the divider to the lower plate.

§ 8. Each experiment was as a rule begun by polishing a metal plate on clean glass-paper or emery cloth. Its contact potential with a standard plate, generally of electrolytically deposited gold washed some hours previously with alcohol, as used by M. Pellat, was then measured. The plate was next subjected to some particular treatment; for instance it was filed, or burnished, or polished on leather or paper, or washed with water, alcohol, or turpentine, or heated in air and oxidized, or not oxidized, or exposed to steam, or oxygen, or fumes of iodine or hydrogen sulphide, or simply left to alter under the influences of the atmosphere and its own molecular forces. Its potential with the same standard plate was again measured, and the change due to the treatment its surface had undergone noted.

§ 9. The metal which requires to be joined to the zinc end of the battery, in order to effect a balance, will be called *positive*. Thus aluminium is positive to zinc and zinc positive to copper. Also, when a plate becomes more positive its potential will be said to *rise*; when more negative, to *fall*. When the "potential" of a plate is mentioned, without other qualification, its contact potential-difference with a standard plate (§ 8) is meant.

§ 10. The results given are not in the order in which they were obtained, but are classified in such a way as to show more clearly their true import, and those of many experiments, which are not mentioned in the text, will be found in the tables.

III. Effects of different Methods of Cleansing the Metallic Surfaces.

§ 11. It seems probable that the very conflicting results obtained by different experimenters for the potential-difference of any given pair of metals, in air, must be due, to a large extent, to differences in the methods by which the surfaces have been prepared. In order to obtain a metallic surface as free as possible from all contamination it is clear that a *hard* polishing agent, such as a clean steel file, or emery-cloth, or glass-paper, must be used, as a softer material leaves

more of itself on the plate. A liquid is quite unsuitable, since every particle of the metal probably retains particles of the liquid adhering to it. Thus the smell of a liquid remains long after the plate appears to be quite dry; and it has been found that such a film as must exist to cause the odour is quite sufficient to alter the potential very considerably.

§ 12. In order to obtain uniformity of action the clean glass-paper or emery-cloth used to polish a plate was fixed on a wooden roller made to revolve with circumferential velocity of about 100 cm. per second. Care was taken to hold the plate so that the scratches, caused by the polisher, should all be parallel. Thus little or no grit could lodge in the surface, which would have occurred had the scratches crossed one another. A record was kept of the nature of the polishing agent in every case. A piece of glass-paper, or other polisher, was seldom used more than once or twice, and was never used for any different metal.

§ 13. M. Pellat found that every change in the smoothness of a surface is accompanied by a change in its contact potential; but as his experiments were limited to metals washed with alcohol, it was of interest to extend them to more general cases. Thus a plate of zinc which had been polished on clean glass-paper, and had therefore a surface sharply scratched in parallel lines, was found to be

0.70 volt

positive to the standard gold plate. It was next burnished with a tool of hardened steel, and with the same standard plate it now gave

0.94 volt.

Two hours later the same plate gave

0.92 volt,

showing that the effect is nearly permanent. By burnishing the plate again a finer polish was obtained. With the same standard the potential is now

1.00 volt,

still further burnishing giving

1.02 volt.

If the zinc be now polished on glass-paper it returns to its original potential. Now steel, of which the burnisher was made, is negative to zinc; hence this rise cannot have been due to particles of steel on the zinc, but must have been caused by some change involved in the smoothing process to which the zinc had been subjected, possibly by *a hardening of the surface layer.*

§ 14. Intermediate states of polish give intermediate values of potential. For instance, if a zinc plate be filed it will not be so sharply scratched as it would be by glass-paper, and of course not so smooth as if burnished. Its potential is also found to be between those of scratched and burnished zinc. Each polishing, with any material, was usually sufficiently thorough to efface the effects of all previous polishings: the potential observed thus depended only on the state of surface produced by the polisher used just before the observation. The results of experiments show that the smoother the surface the more positive it becomes.

§ 15. The generalization given in § 14 is supported by the results of over 100 similar experiments, with a number of metals. The actual results are given in Table I; it will be well, however, to discuss some of these in greater detail than is possible in tabular form.

§ 16. In order to eliminate the use of different polishing agents, I tried the effect of producing different states of surface by rubbing the plate against another of the same metal. Two copper plates which had been polished on glass-paper gave

(a) +0.01 volt,

(b) +0.04 „

with the standard plate. They were next gently rubbed together until parts of each were shiny and now gave, with the same standard,

(a) +0.07 volt,

(b) +0.07 „

In another experiment two coppers which had been polished on medium emery-cloth (Davies's No. 1½) gave, with the standard plate,

(a) -0.11 volt.

(b) -0.06 „

When slightly burnished by rubbing them together, they gave

(a) -0.02 volt.

(b) -0.02 „

Thus, smoothing by mutual friction made both more positive. The amount of the change is not great, either in potential or in smoothness, as it is very difficult to polish copper on copper, but the direction of change is the same as previously found, namely, a smooth surface is more positive than a sharply scratched one.

§ 17. On account of the difficulty of obtaining a burnished surface by simply rubbing two pieces of the same metal together, I tried another form of experiment in which the possible effect of the

material of the polishing agent is eliminated by using the same tool to produce different states of surface. Thus a copper plate was scratched very roughly with a steel tool, and gave

—0.07 volt

with the standard plate, then burnished slightly with the same tool it gave

—0.02 volt. .

It had thus risen 0.05 volt on account of the alteration produced in smoothing its surface. Thus these results also confirm the conclusion given in § 14.

§ 18. It should be noticed that although individual results occasionally appear to conflict, the average value of the potential of any given metal when polished with a certain agent is almost always in proportion to the smoothness of the surface produced. However much one may try, it is impossible to get *exactly* the same state of surface over again, but by taking the average of a considerable number of experiments with one polishing agent, a general value is got which represents the potential given by the plate in a certain state of polish. It is so arranged that the sharply scratched surfaces come at the head of the table, and each succeeding polishing agent in the list produces a smoother surface.

§ 19. It should be observed that with soft metals such as tin, different polishers produce but little change in the contact potential. This, though at first sight apparently contrary to the results for harder metals, is really in accordance with them, for different polishers do *not* produce appreciably different states of polish on a soft metal. This remark applies equally to the fact that a smaller variation was observed with "high conductivity" copper than with the old copper plate, as the old plate was found to be considerably harder than the purer "high conductivity" copper.

§ 20. It is probable that the variation described in this chapter is not directly due to roughness or smoothness, but rather to alteration, either by change in the outer layer of the metal, or in the film of condensed air which no doubt exists on it, of the interface between metal and air.

IV. *Thin Solid Films of Oxides, Iodides, &c.*

§ 21. Among the earlier experiments were many in which the changes due to films of oxide of different thicknesses were studied; indeed this was the primary object of investigation suggested by Lord Kelvin. But constant difficulties and ambiguities presented *themselves*, hindering the interpretation of even the simplest

experiments, and it was found necessary to enlarge the scope of the research in order to remove or explain them.

§ 22. The experiments detailed below are among the most definite of those carried out. They show large changes in potential on account of very thin films of oxide, and prove that the amount of change is dependent on the thickness of the film when below a certain limit.

§ 23. Two plates of cast zinc, which had been carefully polished on medium glass-paper the day before, gave with one another

+0.02 volt,

the upper plate being positive. The upper plate was now taken out and heated on the back by means of a very small blowpipe flame. Its face became slightly blistered, but not much discoloured, and when cold it gave with the other, which had not been altered in any way,

—0.44 volt,

the "minus" indicating that the upper is now negative. This shows a permanent fall of 0.46 volt due to heating in air.

§ 24. A plate of rolled zinc, which gave

—0.04 volt

with another plate of the same material, was heated as in the last experiment, but to a higher temperature, very nearly to its melting point. Its face became a streaky yellowish-brown, slightly purple towards the centre (the brown parts turn purple if moistened). When compared, cold, with the clean plate it now gave

—0.79 volt,

a fall of 0.75 volt. Thus this oxidized zinc plate has practically the same potential as copper. This great fall of potential does not take place by exposure to the atmosphere at ordinary temperatures, unless possibly after the lapse of many years, for the potential of zinc plates which have been left unpolished for many months is only two or three tenths of a volt below that of clean zinc (see § 34).

§ 25. Similar experiments were made with copper. The film of oxide was gradually increased in thickness by repeatedly heating the plate, and the potential measured in each stage of oxidation, the plate being cold. There was a change of about 0.03 volt in the negative direction before the oxide-film became visible, and further increase in the thickness of the film was judged by the gradual change of colour, until the well-known dull purplish-black of massive copper oxide was attained. The potential, measured each time when the plate was cold, gradually fell until it reached a limit of about 0.30 volt

negative to a standard gold plate, which value may therefore be taken as the potential of a mass of copper oxide at 15° C. It may be remarked that there are good reasons, which will be given in Chapter IX, for supposing that oxidation does not commence in air unless the temperature of the plate be raised above 80° C.

§ 26. A plate of copper which had been polished on glass paper and then on wash-leather gave

+0.20 volt,

with a standard gold plate, and was next held in iodine vapour for a short time. It looked moist at first, but quickly became dry. In this state it gave

—0.34 volt,

with the same standard, a change of 0.54 volt in the negative direction. The surface had a dull colour with a whitish "bloom" on it, the tint resembling that of clean copper, though rather darker. Exposure to iodine vapour thus makes the surface of copper nearly as negative as continued exposure to the atmosphere at a high temperature.

§ 27. The potential of a clean zinc plate fell about 0.12 volt when it had been exposed to iodine vapour.

§ 28. The iodine compounds formed on the metallic surfaces were not stable, as the appearance and potential of the plates altered considerably during twenty hours' exposure to the atmosphere.

§ 29. The effects of sulphur fumes, hydrogen sulphide, &c., were tried, but though in most cases considerable changes were observed, the results obtained were not satisfactory.

V. *Atmospheric or Time Effects.*

§ 30. A small variation of the potential is usually found to take place during a short time after the plate has been polished, and before it has settled down to a more or less permanent value. This is partly due to the fact that the plate has been slightly warmed during polishing, and takes some time to come to the atmospheric temperature (see Chap. IX). As regards changes which are not due to variation of temperature, I shall give some typical experiments in the following sections of this chapter.

§ 31. Two plates of "high conductivity" copper were polished on fine glass-paper, and several experiments by burnishing them by mutual friction were then made (see § 16). Forty-seven minutes after the original polishing, their potentials with the standard plate were

(a) +0.035 volt.

(b) +0.045 „

The plate (a) was left in the apparatus all night, that is to say, in a position sheltered from dust, and in air partially dried by the sulphuric acid in the lower part of the case; the potential next morning, 20 hours 3 mins. after the measurements given above, was

+0.045 volt

with same standard plate. It was thus nearly constant. The plate (b) was left in a rack in the laboratory without special protection, with the result that next morning the potential, owing to exposure to the air of the room for about 20 hours, had fallen to

-0.056 volt,

a change of -0.001 volt. The plate (b) was now placed in the apparatus, and remained nearly constant for 2 hours at least. Plate (a) was left in the rack, and its potential fell 0.065 volt in about 2 hours. It is thus evident that at ordinary temperatures in a rather dry atmosphere, and in a place protected from dust and light, the rate of variation of the contact-potential of copper is very small; in an exposed place, however, it is no means negligible.

§ 32. A plate of block tin was polished on glass-paper. Its potential varied with time as follows, the time being counted from the completion of the polishing:—

Hours.	Mins.	Volts.
0	22.....	+0.515
0	27.....	+0.520
0	46.....	+0.535
1	4.....	+0.535
1	23.....	+0.515
3	18.....	+0.495
4	6.....	+0.495
4	42.....	+0.495
71	12.....	+0.465

The plate was kept in the apparatus all the time and its potential taken with a standard gold plate.

§ 33. In another experiment a tin plate polished on the finest glass-paper gave, after 0 hours 15 mins.,

+0.525 volt.

After 23 hours 30 mins. it gave

+0.535 volt.

This experiment was made in June, while the previous one, was made in December. Hence probably the greater rate of variation in the former was due to a moister and less pure atmosphere.

§ 34. An old zinc plate which had probably not been polished for many years gave

+0.37 volt

with the standard gold plate. It was next polished on glass-paper, and gave

+0.75 volt

with the same standard. This shows that the ultimate effect of exposure is to make the potential of zinc fall. I have found that in some cases this fall is preceded by a slight rise, as in the case of tin (§§ 32, 33); but the ultimate effect is in the negative direction.

§ 35. The potential of a silver plate which had been polished on glass-paper remained constant for an hour. The experiment was not continued further.

§ 36. Aluminium becomes gradually negative in air. In one case the potential fell about 0.18 volt in a week. The time-change of this metal, mainly on account of its large temperature-variation (see Chap. IX), is rather difficult to determine.

§ 37. An iron plate which had not been cleansed for two months rose 0.05 volt when polished. Hence the effect of exposure had been to make its potential fall slightly.

§ 38. I have included these results, for which, as for many others like them, I can as yet give no definite explanation, in the hope that they may serve as a basis for future experiment and generalization. The great difficulty in all such experiments on contact electricity is to define the circumstances and, with them, the causes of any given variation.

VI. *Very Thin Liquid Films on Metals.*

§ 39. In the earlier experiments it was found that, as a rule, copper polished on glass-paper or emery-cloth was about 0.20 volt *negative* to the standard alcohol-washed gold plate.

On comparing this value with M. Pellat's results* I found that his value for copper was about 0.20 *positive* to the same standard. I at once determined to try his method of cleansing the plate so as to find if the difference (0.40 volt) were due to that alone. For this purpose a copper plate was carefully polished on glass-paper. It gave, with the standard,

-0.20 volt.

It was then washed with alcohol and allowed to dry in air. It now gave

+0.22 volt

with the same standard. The change due to the treatment is therefore +0.42 volt, and continues permanent many hours.

* 'Ann. Chim. Phys.,' 1881.

§ 40. A plate of thickly oxidized copper giving, with the standard plate,

—0·24 volt

gave, after it had been washed with alcohol, and allowed to dry,

0·00 volt,

a rise of 0·24 volt.

§ 41. A zinc plate, when cleaned on glass paper, gave

+0·61 volt.

It was then washed with alcohol, and when apparently quite dry gave

+0·94 volt,

remaining constant at this value for many hours. The rise in this case is 0·33 volt.

§ 42. The results of many similar experiments made with alcohol and other liquids on gold, silver, zinc, copper, iron, tin, lead, and aluminium will be found in Table II. It is notable that in almost every case the contact-potential of a metal which has been polished on a hard dry material rises at least 0·15 volt in consequence of alcohol washing, and that this change is nearly permanent for many hours after the plate appears to be quite dry.

§ 43. It may be remarked that, as M. Pellat appears to have invariably washed the metal with alcohol before making a measurement of its potential, his results do not apply directly to metals which have been cleaned by means of a hard dry agent only. Even in the experiment which he gives on pp. 79—80 of his paper as a proof that alcohol-washing does not permanently alter the potential of a clean gold plate, he appears to have used a plate which had already been washed with alcohol; his result, therefore, does not prove that alcohol-washing makes no change in the contact-potential of a clean plate, but only that subsequent washings do not alter the state of surface, *i.e.*, that the effect is nearly permanent. This agrees with the results given above and in Table II.

§ 44. The alcohol film, whether in combination with the metal or not, must in any case displace the air from the surface of the plate. Other liquids seem to leave films in much the same way, though the change of potential is different for each liquid; in some cases, *e.g.* turpentine on zinc and copper, it is in the positive direction for the positive metal and in the negative for the other.

VII. *Thick Liquid Films on Metals.*

§ 45. In experimenting with films of sensible thickness the liquid was usually placed on a flat metal plate, its surface-tension being

Table II.—Permanent Changes produced in Contact-potential of Metals by washing with Alcohol and drying in Air.

Metal.	Previous treatment.	Potential with standard gold plate.		
		Before.	After.	Change.
		volt.	volt.	volt.
Copper	Polished on glass-paper	-0·20	+0·22	+0·42
"	" " "	+0·09	+0·29	+0·20
"	Polished on emery-cloth	+0·02	+0·14	+0·12
"	Filed	-0·13	+0·14	+0·27
"	Oxidised	-0·24	0·00	+0·24
Gold	Polished with dry "plate powder"	+0·06
Silver.....	Polished on glass-paper	+0·10	+0·20	+0·10
Iron.....	Dry polished	+0·08	+0·16	+0·08
"	Polished on emery-cloth	+0·17	+0·29	+0·12
"	" " "	+0·17
"	" " "	+0·14
Lead	Scraped thoroughly with a knife	+0·61	+0·80	+0·19
Tin.....	Polished on glass-paper.....	+0·56	+0·66	+0·10
Zinc.....	" " "	+0·61	+0·94	+0·33
Aluminium	" " "	+1·05	+1·10	+0·05
"	" " "	+1·04	+1·07	+0·03
"	" " "	+1·13	+1·28	+0·15
"	" " "	+1·17	+1·26	+0·09

sufficient to retain it if its depth were not more than 0·3 or 0·4 cm. In other cases a shallow metal dish was used. The behaviour of water puzzled me for some time, and made me realize the extreme difficulty of obtaining a clean liquid surface. After some preliminary experiments I found that distilled water which has not been exposed to the atmosphere gives fairly constant results when lying to a depth of 0·2 cm. on one of the standard gold plates. When water on gold formed the one plate and a dry standard gold plate (see § 8) the other, the value found was 0·10 volt, the water being negative. Thus clean water in contact with gold is about 0·10 volt negative to it. The value found is quite different if the water has been exposed to the air for some time, and is usually positive instead of negative to the standard plate.

§ 46. Air expelled from the lungs produces a very marked change in the contact-potential of water. By blowing through a fine glass

tube which dipped under the surface of a layer of distilled water on a gold plate, the potential was changed from

—0·10 volt

with the standard plate to

+0·16 volt,

and remained at this latter value for more than half an hour, showing that the change was not due to a rise in temperature. The curious point is that the change seems to be only in the surface of the liquid; for when most of it was shaken off the potential fell to

—0·05 volt,

although the plate was still wet all over. This appears as if the alteration were due to a surface film of oily or dusty matter on the water. It was found that distilled water which had been exposed to the air of the room for some time gave the same value as water which had been breathed through.

§ 47. A lead plate and a flat circular leaden dish were scraped clean with a knife, and next morning, when compared with each other, they gave

+0·10 volt,

the plate being positive to the dish. Water was now poured into the dish until it was nearly full, 0·4 cm. deep, the potential remaining the same; i.e., the lead plate gave with the water in the leaden dish,

+0·10 volt,

water in leaden dish being negative.

§ 48. The plate and dish were again scraped, and this time they gave

+0·05 volt,

the plate being positive. Water was next poured into the dish, which gave, with the dry lead plate,

+0·09 volt,

the water in the dish being negative as before. The dish was now removed, and a standard gold plate put in its place; with this the lead plate gave

+0·52 volt,

the lead being positive. Thus water in contact with lead is about 0·9 volt negative to dry lead.

§ 49. Turpentine seems to produce opposite effects on zinc and copper, i.e., copper becomes more negative, and zinc more positive,

when wet. Thus copper and zinc which had been dry-polished gave with one another

0·77 volt,

zinc positive.

With the zinc wet with turpentine,

0·96 volt.

And with both zinc and copper wet about

1·20 volts.

Their potential difference increased somewhat as the turpentine dried up; and when apparently quite dry its value was still at least

1·20 volts,

the plates, however, smelt strongly of turpentine, showing that an invisible layer remained on their surfaces.

§ 50. A crystal of copper sulphate gave, with a standard gold plate

+0·02 volt,

copper sulphate positive. This result was obtained in connection with some experiments on the contact-potential of an aqueous solution of copper sulphate. It was found that such a solution in a copper dish gave about +0·07 volt with a dry standard gold plate, the solution being positive. Blotting-paper saturated with copper sulphate gave about +0·10 volt with the same standard plate. As no special precautions were taken in these experiments to obtain a perfectly pure liquid surface, one cannot, especially in view of the changes described in § 46, be quite certain that they represent the true potential of copper coated with a solution of its sulphate. They agree, however, with Professors Ayrton and Perry's values for saturated and non-saturated solutions.

§ 51. One of the standard gold plates which had been polished with Hollis's plate powder used dry gave, with a similar plate which had a layer of alcohol on it,

—0·13 volt.

When the alcohol had dried up the value was

—0·05 volt,

the polished plate being negative as before.

§ 52. The conclusion which I draw from the experiments described in this and the preceding chapter and in the tables is, that a layer of liquid on a metallic surface does not give, with a metal separated from it by air, a definite potential-difference of its own, as in similar *circumstances* a *solid* conducting film would do, but merely adds a

certain amount to that of the plate on which it lies. Thus two different metals coated with layers of the same liquid do not, as a rule in air, give zero potential-difference, but usually give nearly the same potential-difference as the dry metals. For instance, the potential of lead with an alcohol layer 0.1 cm. deep on it is about 0.13 volt higher than that of dry lead, while that of wet copper is about the same amount higher than the average value for dry copper. There is no tendency shown for a liquid film to take up a definite potential independent of that of the metal on which it lies, with any metal separated from it by a dielectric, as a solid conducting film would do. This seems to be the most important distinction between solid and liquid conductors, and it is in accord with what is known of voltaic cells; for if the potential-differences in the chain copper-water-zinc were equal and opposite to that of zinc-copper, as copper-iron-zinc is to zinc-copper, we should have no electromotive force in the circuit, when the materials are all at one temperature. In most of my experiments the type is copper-water-nonconductor (air)-water-zinc, and not copper-water-zinc; the members of the first and last pairs are in contact, while a non-conductor intervenes between the two free water surfaces, these being able to take up their natural contact-potential-differences with the metals they touch; but the sum of the potentials, copper-water and water-zinc, is not equal to copper-zinc, thus proving that the two free surfaces of water are not at the same potential. This is directly demonstrated by the experiments of Professors Ayrton and Perry, and by the results given in Chaps. VI, VII, and X of this communication.

If the intervening layers of non-conductor be removed by joining the liquid surfaces so that there is but one mass of liquid between the plates, conduction at once tends to reduce the whole liquid to the same potential, leaving the contact-potential differences, now unbalanced by the removal of the non-conducting medium which was capable of sustaining the stress, to act as external electromotive force. This then shows the connection between contact-potentials, measured electrostatically by the method described in this paper, and the electromotive force of a voltaic cell.

§ 53. The contact-potential of a liquid with a metal is clearly, if the air-potentials be neglected, the difference between the potential of the dry metal and that of the metal when wet with the liquid; the same standard plate being used as zero of potential in both cases. The results given in the tables must no doubt be in some cases complicated by the formation of solid compounds in the interface between liquid and metal, so that the liquid is no longer in contact with clean metal, and the liquid must also displace any film of condensed air which may exist on the metal. The latter influence will be discussed in Chaps. VIII and X.

Table III.—Potential of Metals covered with visible layer of Alcohol.

Metal.	State of surface.	Potential with standard gold plate.		
		Dry.	Wet.	Change.
Copper	Filed; wet with alcohol.....	volt. -0·13	volt. +0·19	volt. +0·32
Gold	Dry-polished; wet with alcohol	+0·13
Silver.....	Glass-paper polished.....	+0·10		
	Wet, 0·05 cm. deep.....	..	+0·18	+0·08
	„ thin film	+0·19	+0·09
Lead	Scraped thoroughly	+0·61		
	Wet, 0·1 cm. deep.....	..	+0·74	+0·13
	„ thin film only..	+0·74	+0·13

Table IV.—Change in Contact-potential of Zinc and Copper due to Layers of Turpentine, Rosin Oil, and Indiarubber Solution.

Liquid.	Metal.	Potential, volts.		Remarks.
			Change.	
Wet with turpentine	Copper, polished on glass-paper	..	-0·11	<i>Note.</i> —The plates were compared with one another, and not with the standard plate. Hence only the <i>change</i> of potential is given.
	Zinc, polished on glass-paper	..	+0·20	
Wet with rosin oil	Copper, polished on glass-paper	..	-0·12	
	Zinc, polished on glass-paper.	..	-0·02	
Wet with indiarubber solution	Copper, polished on glass-paper	..	+0·02	
	Zinc, polished on glass-paper	..	+0·07	

VIII. Films formed on Metals by Gases.

§ 54. The potential of a metal is usually altered by soaking the plate in a gas other than air. If the gas be oxygen, this alteration is as a rule only temporary, and apparently depends on the formation of a surface film, or rather on a change in the film which doubtless *already exists*. It will be shown that the change of potential of zinc

due to soaking in oxygen is nearly equal to that of copper; hence the film of oxygen acts like a liquid film (Chap. VII), but its effect is less permanent. Previous experimenters, except Lord Kelvin, appear to have neglected the existence of these films, and to have looked only to the nature of the body of gas between the plates. Of course I do not here allude to the "double-layer" which has been offered as an explanation of the phenomenon, but to a layer in mechanical and electrical contact with the metal. If the contact-potential of two metals immersed in a gas were the sum of the potential-differences between each metal and a skin of gas close to it, we should have no slope of potential in the body of the gas between the plates. That a slope does exist, however, is proved by Lord Kelvin's earliest experiments with the divided ring of copper and zinc. In this connection Dr. Bottomley's research on contact-electricity in high vacua* is of great importance as showing that the volta-potential of metals is not sensibly different in different gases so long as the metals are not chemically affected, and is not sensibly altered by a great reduction of pressure. The body of the gas may possibly have some influence, but the variations which have frequently been attributed to it may usually be more satisfactorily explained as being due to change in the film in contact with the metallic surface. In the case of a gas which acts vigorously on the metal at ordinary temperatures, the film is permanent, and is probably a solid compound; in other cases it is not permanent, and hence probably not solid.

§ 55. In 'Nature' for 1881, Lord Kelvin describes some very important experiments on this subject. As these appear to be but little known I shall give some extracts from his paper before describing my own results. Under the date November 23, 1880, Lord Kelvin says:—"I have found that a dry platinum disc, kept for some time in dry hydrogen gas, and then put into its position in dry atmospheric air in the Volta-condenser, becomes positive to another platinum disc which had not been so treated, but had simply been left undisturbed in the apparatus. The positive quality thus produced by the hydrogen diminishes gradually, and becomes insensible after two or three days. P.S.—On December 24, 1880, one of the platinum plates in the Volta-condenser was taken out; placed in dried oxygen gas for forty-five minutes; taken out, carried by hand, and replaced in the Volta-condenser at 12.30 on that day. It was then found to be negative to the platinum plate, which had been left undisturbed. The amount of the difference was about 0.33 of a volt. The plates were left undisturbed for seventeen minutes in the condenser, and were tested again, and the difference was found to have fallen to 0.29 of a volt. At noon on the 25th they were again

* 'Brit. Assoc. Report,' 1885.

tested, and the difference found to be 0.18. The difference had been tested from time to time since that day, the plates having been left in the condenser undisturbed in the intervals. The following table shows the whole series of these results:—

Time.	Electric difference between surfaces of a platinum plate in natural condition, and a platinum plate after 45 mins. exposure to dry oxygen gas.
Dec. 24, 12.30 P.M.....	0.33 of a volt.
„ 24, 12.47 „	0.29 „
„ 25, noon	0.18 „
„ 27, „	0.116 „
„ 28, 11.20 A.M.....	0.097 „
„ 31, noon	0.047 „
Jan. 4, 11 A.M.....	0.042 „
„ 11, 11.40 A.M.....	0.020 „

After detailing some experiments in which the plates were coated with the gases by electrolysis, Lord Kelvin concludes: “Thus in the case of polarization by oxygen, as well as in the case of polarization by hydrogen, the effect of exposure to the dry gas was considerably greater than the effect of electroplating the platinum with the gas by the electromotive force of one volt.”

The large effects on contact-potential produced by films formed from gases are clearly shown in these experiments of Lord Kelvin's. It is well known that platinum and other metals have the property of occluding large quantities of gas in their surface layers, and that the condensed gas is possibly in the liquid state, which would account for the similarity between the effects of liquids and of gases on metals.

§ 56. The plates with which my first experiments on this subject were made were of “high conductivity” copper. They had been polished on fine glass-paper 5 hours before, and their mutual potential had remained constant at

0.02 volt

for 4 hours. The lower plate was then put into a glass vessel, into which oxygen gas was admitted from a cylinder, and the oxygen, which was of Messrs. Brin's manufacture, guaranteed 93—95 per cent. oxygen (nitrogen is usually the only impurity), was allowed to stream through the glass vessel containing the plate for some minutes, and the exit and inlet of the vessel were then closed. Forty-five minutes later the plate was taken out and its potential again measured with the other plate, which had remained in air during the interval. *Counting* time from the moment at which the plate was taken out of

the oxygen, the potential varied as shown below, the oxygenized plate being positive to the other in all cases:—

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	3.....	0·12
0	19.....	0·08
0	30.....	0·06
17	20.....	0·03

Thus the effect of increasing the proportion of oxygen in the surface film was to make the copper more positive.

§ 57. This variation is in the opposite direction to that found by Lord Kelvin for platinum which has been soaked in oxygen. In order to make sure that this difference was not the effect of some impurity in the oxygen, I repeated his experiment, obtaining the same result as he had obtained. This shows that the result given above was not likely to be due to an impurity which had influenced the action of the oxygen.

§ 58. Without any further treatment the same copper plate was placed in the bell-jar and the oxygen admitted. After 45 minutes it was taken out and its potential again measured by comparison with the other plate. The results are given in the following table, time being counted from the moment the plate was taken out of the oxygen:—

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	2.....	0·13
0	10.....	0·08
0	21.....	0·07
0	29.....	0·06

The experiment was discontinued before the plate had returned to its original value; but it would no doubt have done so in a few hours, for the amount of change, and its rate, are almost exactly the same as in the former experiment (see § 56).

§ 59. In the following experiment the conditions were somewhat varied. A copper plate was polished on glass-paper. It gave, with a standard copper plate,

—0·05 volt.

A jet of oxygen was now sent against its surface for 2 or 3 minutes, and with the same standard plate it now gave

—0·06 volt.

It was then left in oxygen for 25 minutes, and on being taken out gave

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	4.....	+0·060
0	13.....	+0·035

which shows that its immersion had raised its potential 0·12 volt. I now warmed it with a soldering-bolt applied to its back. When about 47° C. (see Chap. IX) it gave

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	16.....	+0·020

It was again warmed slightly, and when at about 30° C. gave

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	42.....	-0·010
4	48.....	-0·040

When 16° C.,

its potential thus coming back to very nearly the original value.

This experiment shows: (1) that the change requires considerable time; for even a fairly strong jet of oxygen playing on the plate for 2 or 3 minutes produces no appreciable effect, while 25 minutes in still oxygen causes a rise of 0·20 volt; (2) that gentle heating does not produce a rise, as it would do with clean unoxxygenized copper (see Chap. IX), but it must be remembered that the copper was, when heat was applied, already above the potential to which heat alone would have raised it; hence this experiment does not show any connection between temperature-variation and density of oxygen film, as might at first sight be supposed.

§ 60. A zinc plate which had been polished on glass-paper gave with a standard copper plate

0·81 volt, zinc positive.

It was then put into oxygen and left for 15 minutes. After being taken out, its potential was again measured with the same standard plate, and was as follows:—

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	2.....	0·89
0	10.....	0·87
4	18.....	0·86
4	35.....	0·85

This shows that zinc also is more positive after immersion in oxygen.

§ 61. The same zinc plate was again polished on glass-paper and gave

+0·73 volt

with the standard copper. A short time later it gave

+0.70 volt.

It was now put into oxygen for 10 minutes, and after being taken out gave

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	3.....	+0.80
0	13.....	+0.78
17	1.....	+0.74

§ 62. A tin plate was polished on clean glass-paper, and gave with a copper plate

+0.40 volt,

tin being positive. Fifteen minutes later the potential of the plates had not altered. I now put the tin into the bell-jar and turned on the oxygen. After it had soaked for 47 minutes it was taken out and compared with the same copper plate. It gave

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	5.....	+0.32
0	13.....	+0.32
0	35.....	+0.35

Thus the variation of tin appears to be in the negative direction, like that of platinum.

§ 63. A silver plate, polished on glass-paper, gave with a standard copper plate

-0.04 volt,

and remained constant during an hour. It was then put into oxygen for 15 minutes, and when taken out its potential was found to be

Time.		Contact-potential.
Hrs.	Mins.	Volt.
0	3.....	+0.02
0	19.....	-0.01
0	40.....	-0.02
0	45.....	-0.02
0	59.....	-0.03

Thus by immersion in oxygen for 15 minutes it had risen 0.06 volt and had fallen to nearly its original value in an hour in air.

§ 64. Silver polished on clean "fine" glass-paper gave with a standard copper

-0.095 volt.

After it had been 24 minutes in oxygen it gave in air

+0.015 volt,

and 12 minutes later

+0.010 volt,

showing that the potential had in 24 minutes in oxygen become 0.110 more positive.

§ 65. It is noticeable that the amount of change in these experiments on silver is to some extent proportionate to the time of exposure to oxygen. Thus, in § 63, 15 minutes in oxygen caused a rise of 0.06 volt; while, in § 64, 24 minutes in oxygen caused a rise of 0.11 volt, but there is no doubt a limit to the change.

§ 66. In searching for an explanation of the temperature-variations described in Chapter IX, I compared them with those given above. In the case of copper, the oxygen-film variation is in the same direction as the temperature-variation of copper in air, which suggests the possibility of the latter being caused by an increase of the proportion of oxygen in the film at higher temperatures on account of a greater attraction between the elements. The same reasoning holds as regards zinc and tin, but the results for silver are in direct opposition; while the further experiments described in Chapter X, which show that the temperature-variations exist in cases where air is entirely excluded from the metallic surface, render such an explanation very doubtful. Probably, therefore, the temperature-variation is the more general of the two, it being a change in contact-potential of the metals, which, if they are exposed to a gas, may be complicated by alteration of the surface-film.

IX. *Temperature Variations.*

§ 67. A large number of determinations were made of the variation of contact-electricity with the temperature of the conductor. This was done by heating one plate while the other was kept cool, and their potential-difference was measured from time to time as the warm plate was cooling, their temperatures being observed at the same time. In the diagrams, the abscissæ represent temperature and the ordinates potential; each curve, therefore, shows the temperature-variation of the contact-potential of a particular metal. For instance, the potential represented by the point which corresponds to 16° C. on the gold line is called zero in this and the other chapters of the present communication. Thus a standard gold plate (see § 8) at 40° C. is 0.04 volt negative to one at 16° C., and an aluminium plate at 40° C. is 0.10 volt, positive to aluminium at 16° C., or 1.20 volts positive to a gold plate at 16° C.

It must be remembered that unless stated otherwise these varia-

tions are for metals in air. In the experiments described in Chapter X, however, the metallic surfaces were protected by solid non-conducting films, and were not in contact with the atmosphere; nevertheless, temperature-variations were found, which in the case of silver were actually larger than those which took place in air.

§ 68. One of the copper plates used was hollow and could be filled with water and a thermometer inserted, but with the other plates other methods of measuring temperature had to be adopted. In some cases the temperature was measured thermo-electrically, while in others a simpler and more rapid mode of measurement was used, which, though not very accurate, is quite reliable within certain limits.

§ 69. By touching the back of the plate I found that its temperature could be judged as "tepid," "slightly warm," "warm," "very warm," and so on. It was found experimentally that these terms correspond to constant temperatures; or rather that each term denotes a small range of temperature, the middle point of which may be taken as corresponding to the term. In determining the values of these terms a plate was used in which a thermometer was inserted. One observer touched the plate with the tips of the first and second fingers and judged its state, naming it by one of the terms, "warm," "tepid," &c.; the other observed the thermometer, and the temperatures found to correspond to each term are as follows :—

Cold.....	16° C.
Quite cool	24
Cool.....	28
Rather cool.....	30
Tepid.....	35
Slightly warm	40
Warm	47
Very warm.....	50
Hot	53
Very hot.....	57
Too hot to touch continuously.....	63
Too hot to touch for more than one second ..	73

This method of measuring temperature is rough and ready, but since the possible errors are within limits of a very few degrees, one only requires to take the average of a considerable number of results in order to arrive at a very fair approximation to the true values. In experiments on contact electricity in air there are so many possible causes of disturbance that extremely accurate measurement of the temperature is of little use, especially if it require that much time be spent over each reading.

§ 70. As a rule the upper plate was heated, in order that the lower plate might not be affected by draughts of hot air, as would have been the case if the lower had been hot and the upper cold. Sometimes, during the time of cooling, the upper part of the apparatus, including of course the upper plate, was removed after each observation and was replaced only the moment before the next. In other experiments the upper plate was merely drawn up as far as possible (about 10 cm.) to prevent its warming the lower one. The temperature of the lower plate was also observed, but as a rule it varied only a very few degrees.

§ 71. At first I used to apply a hot soldering bolt to the back of the plate in order to heat it, but latterly I heated two or three small blocks of tinned copper and placed them on the back of the upper plate. By this second method it was possible to observe the variation of the potential during the rise as well as the fall of temperature.

§ 72. In attempting to determine the temperature-coefficient of copper we were long baffled by curious anomalies. Sometimes the plate was positive when hot, other times negative, and occasionally it did not vary at all. The clue to this was found in observing that during one experiment while the copper was cooling it was at first positive, then negative, and then it gradually became positive again, though never quite reaching its original value.

Now it had been found that copper oxide is negative to copper, and that it became temporarily more negative when hot; hence it was guessed that the successively positive and negative variation must be due to hot clean copper being positive to cold copper, but that it had finally become oxidized and therefore negative whether hot or cold, the small permanent change being due to the thin coating of oxide formed.

§ 73. The copper was heated much more gently next time, and gave the expected result that clean copper becomes rapidly more positive as its temperature rises, and that, on cooling, its potential returns to its original value unless the temperature has exceeded a certain limit. If this limit has been exceeded its potential rapidly becomes negative and does not return to its original value.

§ 74. At ordinary atmospheric temperatures the surface of clean copper remains for a long time almost unaltered either visibly or electrically (see § 31), and the film which ultimately forms on the surface cannot be pure copper oxide, because the potential of tarnished copper is higher than that of copper oxide obtained by heating in air. If, however, the temperature of the copper is raised to about 80° C. it immediately begins to oxidize, though heating to a temperature below this limit does not rapidly produce any permanent change. *Thus there is, as it were, an ignition point for copper and oxygen in*

air; below it, little action takes place; above it, combination proceeds vigorously.

§ 75. I shall now give a specimen experiment. The two standard gold plates gave

—0·02 volt,

the minus sign indicating that the upper plate is negative. I now heated the upper: when “tepid,” i.e., about 35° C., it gave with the gold plate

—0·045 volt.

It was next heated further until “very hot” (57° C.) and gave

—0·06 volt.

When it had cooled down to “tepid” (35° C.), it gave

—0·04 volt.

When “cool” (28° C.),

—0·03 volt.

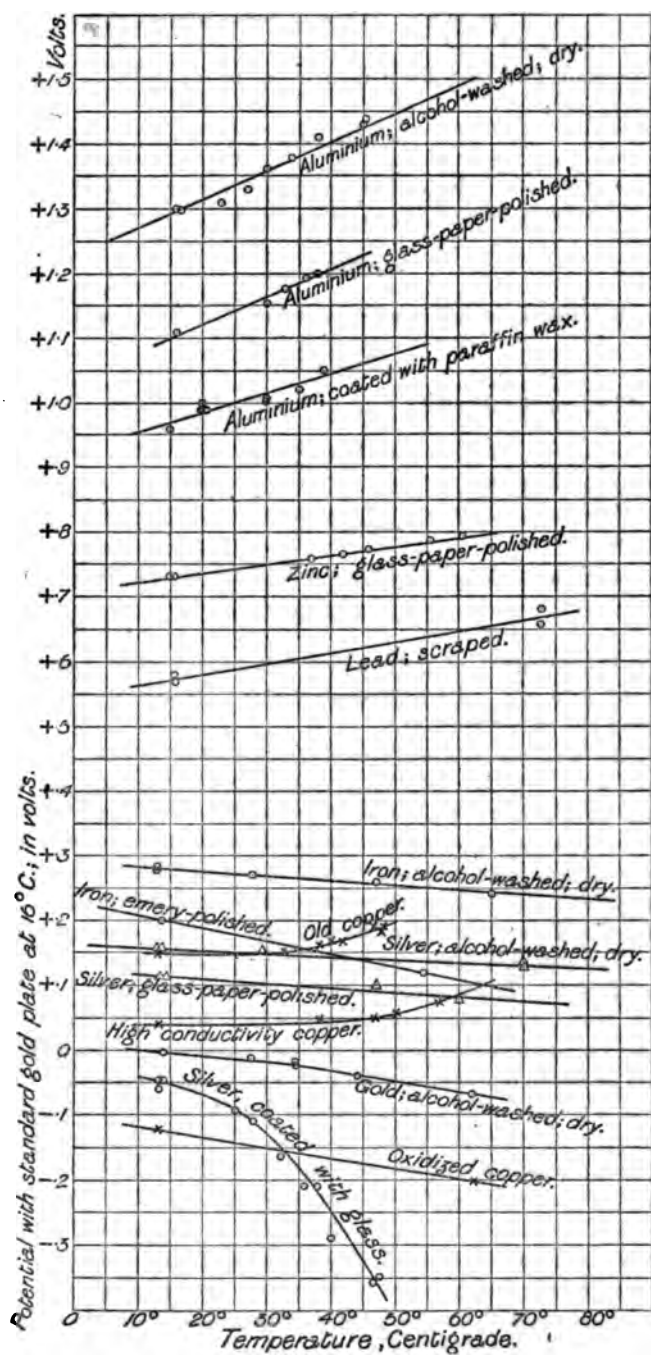
Some hours later, when both plates were cold, they gave as at first

—0·02 volt.

Hence the potential of gold which has been washed with alcohol and allowed to dry falls temporarily about 0·0016 volt per degree centigrade rise of temperature.

§ 76. Most of the temperature experiments on copper were made with the hollow plate previously mentioned, which was filled with hot water in which the bulb of a thermometer was placed. In many of the experiments on zinc, and also on aluminium, a thermo-electric arrangement was used, and the results obtained with it do not differ materially from those obtained by the above method (§ 69). These and other details are noted on the diagram.

§ 77. The diagram gives the temperature-variations of all the substances studied. The curves in it are plotted by taking the results for each metal of those experiments which are most free from all complication or cause of doubt. If the curves be prolonged in the direction of lower temperature they appear to meet somewhere below —200° C., and probably asymptotically to the line representing 0·4 volt positive to standard plate at 16° C. Within their range they show contact-potential-differences diminishing with lowered temperature. The only apparent exceptions are clean copper, and silver coated with glass; but both their curves are distinctly bent between 16° C. and 50° C., so that probably they are directed towards the same point as the others at lower temperatures. Thus it appears that at about —200° C. the contact-potential-differences of metals



The copper plate was now gently heated with a bolt until it was hot enough to melt paraffin-wax; the temperature, about 50°C. , required for this is not sufficient to cause sudden permanent change of the copper surface (see § 74). Paraffin-wax was then poured on, and the plate was filed with its own file while covered with molten wax. Thus the fresh surface exposed by the filing came directly into contact with the wax. More wax was poured on and the filings drained off, the plate remaining well covered with wax all the while. It was then allowed to cool, and gave with the bare zinc plate

+0.555 volt,

zinc being positive as before. Thus the change due to substituting paraffin-wax for air next the copper is not more than +0.100 volt. I now waxed the zinc in exactly the same way. When it was quite cool it gave with the waxed copper

+0.602 volt,

showing that waxing the zinc had raised its potential

+0.047 volt.

So, on the whole, the substitution of wax for air on both copper and zinc had only decreased their mutual potential by

+0.053 volt

and the potential of the waxed plates remained nearly constant for several hours. The changes due to waxing the plates as given above were confirmed by the independent comparison of each plate with the standard gold plate. It does not follow that even the small changes which did occur were due solely to the substitution of wax for air, for they may have been caused by slight changes in the surface on account of the filing.

§ 80. An aluminium plate was coated with wax in the way described in § 79, a knife being used to scrape the surface under the molten wax. When cold, this waxed plate gave with a bare zinc one

+0.36 volt,

which is about the usual value for *bare* aluminium and zinc. The removal of the air had therefore not appreciably altered the potential.

§ 81. I now warmed the waxed aluminium slightly. Its potential with the zinc varied as follows :—

47° C.	+0.41 volt.
35° C.	+0.37 „
28° C.	+0.35 „

This gives a variation of about

0.0032 volt per degree centigrade,

which is nearly the same as the temperature-variation of bare aluminium in air.

§ 82. A plate of silvered glass used with the glass side facing a standard plate gave almost the same potential as clean silver in air. This plate, which was practically silver coated with glass, gave a temperature-variation larger than that of silver in air. In this case we have glass in contact with the silver surface instead of air, but the change does not alter the potential. It may be mentioned that the back of the silver film was painted black, and not coated with glass; but this is of small consequence, since it has been proved by experiment that the condition of the back of a plate does not sensibly affect the volta contact-potential; or, more generally, that if parts of a plate be in different conditions, the potential observed will be the mean of the potentials of the different parts, the importance of each part being proportional to its capacity.

§ 83. On account of the great attraction of sodium for oxygen, it seemed of interest to measure its potential in circumstances which excluded that gas from the surface of the metal. In order to effect this two pieces of thin sheet-glass, each about 6 cm. square, were put into a dish of melted paraffin-wax together with some clean sodium, and a large drop of the sodium was put between the plates of glass and squeezed out into a small plate of 2 or 3 square cm. area. The glass plates, with sodium between them, were taken out of the melted wax and allowed to cool. Since the glass plates were of much larger diameter than the sodium, the edges of the latter were protected by the wax which filled up the space between the plates not occupied by sodium. The flat faces of the sodium were apparently in contact with the glass. The sodium was connected to the electrometer by a fine copper wire. The greater part of the sodium surface was bright or only slightly tarnished, and it remained in almost the same condition for many days, being protected by the glass plates and by the wax which filled the space between them unoccupied by sodium. The first measurements gave sodium

2·86 volts

positive to a tarnished zinc plate, *i.e.*, about

3·56 volts

positive to the standard gold plate. This potential gradually decreased.

§ 84. Experiments were made to make sure that the result was not due to temporary electrification of the glass. For instance I breathed on the glass, causing a conducting layer of impure water to form on its surface. Repeated measurements, made by the usual method, showed that the potential at once fell to a small fraction of

a volt, but slowly rose again to nearly its original value as the film evaporated. This shows that the electrification was not a temporary one of the glass surface, for that would not have returned to a definite value. Heating the plate by radiation or washing the glass with benzol caused the potential to rise further, but in no case was the potential quite so high as when the plate was first formed. An even more convincing proof that the potential measured was really that of the sodium, was found in the fact that the sensibility of the apparatus was such as would be given by a plate the size of the sodium. If the electrification had been on the whole surface of the glass, the sensibility, on account of the larger surface, would have been at least ten times as great as that observed.

§ 85. The experiments described in this chapter show that (i) when two metals are coated with the same non-conductor, such as wax or glass, their potential is not sensibly different from that of the bare metals in air; (ii) that temperature-variation still takes place, though air be excluded. These results seem to prove that gaseous films play no *essential* part in the phenomenon.

“On the Rotation of Plane of Polarisation of Electric Waves by a Twisted Structure.” By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord RAYLEIGH, F.R.S. Received February 14,—Read March 10, 1898.

In my previous papers* I have given accounts of the double refraction and polarisation of electric waves produced by various crystals and other substances, and also by strained dielectrics. An account was there given of the polarisation apparatus with which the effects were studied. In the present investigation effects had to be studied which were exceedingly feeble. The apparatus had, therefore, to be made of extreme sensitiveness; but the secondary disturbances became at the same time more prominent, and the great difficulty experienced was in getting rid of these disturbances.

In one of my communications I alluded to the fact that these secondary disturbances are to a great extent reduced when the radiators are made small. The advantage of a large radiator is the comparative ease with which the receiver can be adjusted to respond to the waves, but this advantage is more than counterbalanced by the increased difficulty with the stray radiation and other disturbances.

* “On the Polarisation of the Electric Ray by Double-refracting Crystals,” ‘Journal of the Asiatic Society of Bengal,’ May, 1895, and “On a New Electropolariscope,” ‘The Electrician,’ December 27, 1895.

On the other hand, with small radiators, the difficulty is in the proper adjustment of the receiver. It then becomes necessary to have very exact adjustments of the receiver, both as regards the pressure to which the sensitive spirals are subjected and the E.M.F. acting on the circuit. It is only after some practice that the peculiarity of each receiver is properly understood, when it becomes easy to make the necessary adjustments by which the receiver becomes quite certain in action. For various reasons the radiations emitted by small radiators are more favourable for work requiring great delicacy.

In order that the surface of the radiator should be little affected by the disintegrating action of the sparks, I use a single spark for producing a flash of radiation. There used to be, however, some uncertainty from a discharge occasionally failing to be oscillatory. The cause of this uncertainty is ascribed to the deposit of dust on the sparking surface. For greater certainty of action some observers immerse the radiator in oil. The use of oil is under any circumstances troublesome. This is specially so in polarisation experiments, when the radiator has to be placed in different azimuths. I have for these reasons avoided the oil-immersion arrangement, and have tried to secure certainty of oscillatory discharge without this expedient. Attention was specially paid to the coil and the primary break. A radiator has also been constructed which is found to be extremely efficient. It consists of two platinum beads, each 2 mm. in diameter, separated by 0.3 mm. spark-gap. There is no interposed third ball. This radiator, though kept exposed for days without any protecting cover, was yet found to give rise to a succession of effective discharges without a single failure. I even went so far as to pour a stream of dust on the radiator, in spite of which severe treatment, the sparks were found to be quite effective in giving rise to electric oscillation.

The receiver, too, is perfectly certain in its action, and various degrees of sensitiveness may be given to it. In the following experiments, the sensitiveness had to be very greatly enhanced, and this, as alluded to above, was secured by proper adjustments. The secondary disturbances were got rid of by careful screening. But one serious difficulty was encountered at the very outset, in the failure of the polariser to produce *complete* polarisation. In my first experiments on polarisation (the receiver then used not having been very sensitive), polarisers made of wire gratings were found effective. But in my later experiments with still more sensitive receivers, I found that, owing probably to the want of strict parallelism of the wires and the difficulty of *exactly* crossing the analyser and polariser, it was impossible to produce total extinction of the field. I then made a polariser and analyser by cutting parallel slits out of two

square pieces of thick copper. When the square pieces were adjusted with coincident edges, the analyser and polariser were either exactly parallel or exactly crossed. This improvement enabled me to carry out successfully some of the more delicate experiments. In the present course of investigation the sensitiveness of the receiver had to be raised to a still higher extent, and it was found that the polariser hitherto found efficient failed to produce complete polarisation, so that even when the polariser and the analyser were exactly crossed the non-polarised portion of radiation was of sufficient intensity to produce strong action on the receiver.

In the paper "On the Selective Conductivity exhibited by some Polarising Substances"* I described a book-form of polariser, when an ordinary book was shown to produce polarisation of the transmitted beam, the vibrations parallel to the pages being absorbed, and those at right angles transmitted in a polarised condition. The advantage of this form of polariser was that the extent to which the rays were polarised depends on the thickness of the polarising medium. The rays could thus be completely polarised by giving the medium a sufficient thickness, this thickness being determined by the intensity of the radiation used and the sensitiveness of the receiver. The necessary thickness of the book-polariser may be materially decreased by making the book consist of alternate leaves of paper and tinfoil. The book being then strongly compressed, blocks of suitable size are cut out to form the polariser and the analyser. Each of these blocks is then enclosed in a brass cell, with two circular openings on opposite sides for the passage of radiation. The size of the polariser I use is 6×6 cm., with a thickness of 4.5 cm.; the aperture is 4 cm. in diameter. These polarising cells I find to be quite efficient; when two such cells are crossed, the field is completely extinguished.

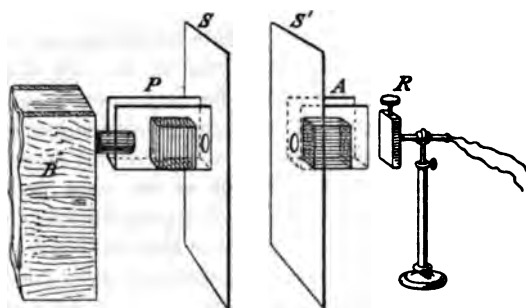


FIG. 1.—Polarisation apparatus. B, the radiating box; P, the polariser; A, the analyser; S, S', the screens; R, the receiver.

* 'Roy, Proc. Soc.,' vol. 60.

The diagram explains the general arrangement of the apparatus, mounted on an optical bench. The spark gap of the radiator is horizontal. The polariser, with the leaves vertical, is placed on a shelf attached to a screen of thick brass plate 35×35 cm. In the centre of the plate there is a circular opening 4 cm. in diameter; this aperture may be varied by a series of diaphragms. There is a second similar screen with a shelf for the analyser, which is placed with the leaves horizontal. Behind the analyser is the receiver.

In the space between the brass plates is placed the substance to be examined. Previous tests are made to see whether all disturbing causes have been removed. The sensitiveness of the receiver is occasionally tested by interposing one's fingers at 45° between the crossed polariser and analyser; this should, by partially restoring the field, produce strong action, provided the receiver is in a fairly sensitive condition.

Care should be taken that there are no metallic masses between the screens, as reflection from metals is found to produce "depolarisation," the rays being then elliptically polarised. The substance to be examined should not, for very delicate experiments, be held by the hand, owing to the disturbing action of the fingers. It is preferable to have the substances supported on stirrups made of thin paper. The above are some of the main precautions to be taken in carrying out the following experiments, where the effects to be detected are very small and therefore likely to be masked unless all disturbing causes are carefully excluded.

I have in a previous communication made mention of the double refracting property of fibrous substances like jute. The field is restored when a bundle of jute is placed at 45° between the crossed polariser and analyser. There is, however, no depolarisation effect when the axis of the bundle is parallel to the direction of the ray.

I now took three similar bundles, A, B, and C, of parallel fibres of jute 10 cm. in length and 4.5 cm. in diameter. No change was made in the bundle A, which was kept as a test one. The bundles B and C were then twisted, B in a right-handed direction and C in a left-handed direction.

The interposition of the untwisted bundle A between the crossed polariser and analyser did not produce any effect, but strong action was produced in the receiver when the bundles, twisted to the right or to the left, were so interposed. It thus appeared as if the twisted structures produced an optical twist of the plane of polarisation.

The further experiments to be described below may be of some interest in connection with the optical rotation produced by liquids. Here two different classes of phenomena may be distinguished:—

- (1) The rotation induced by magnetic field; this rotation among

other things is dependent on the direction and intensity of the magnetic field, and is doubled when the ray is reflected back.

(2) The rotation produced by saccharine and other solutions, when the rotation is equal in all directions and simply proportional to the quantity of active substance traversed by the ray; the rotation in this case is neutralised when the ray is reflected back.

The difficulties in the way of explaining the rotation produced by liquids are summarised in the following extract.

"It is, perhaps, not surprising that crystalline substances should, on account of some special molecular arrangement, possess rotatory power, and affect the propagation of light within the mass in a manner depending on the direction of transmission. The loss of this power when the crystalline structure is destroyed, as when quartz is fused, is consequently an event which would be naturally expected, but the possession of it in all directions by fluids and solutions, in which there can not be any special internal arrangement of the mass of the nature of a crystalline structure, is not a thing which one would have been led to expect beforehand. To Faraday it appeared to be a matter of no ordinary difficulty, and I am not aware that any explanation of it has ever been suggested. It is just possible that the light, in traversing a solution in which the molecules are free to move, may, on account of some peculiarity of structure, cause the molecules to take up some special arrangement, so that the fluid becomes as it were polarised by the transmission of the light, in a manner somewhat analogous to that in which a fluid dielectric is polarised in a field of electrostatic force."*

In order to imitate the rotation produced by liquids like sugar solutions, I made small elements or "molecules" of twisted jute, of two varieties, one kind being twisted to the right (positive) and the other twisted to the left (negative). I now interposed a number of, say, the positive variety, end to end, between the crossed polariser and analyser; this produced a restoration of the field. The same was the case with the negative variety. *I now mixed equal numbers of the two varieties, and there was now no restoration of the field, the rotation produced by one variety being counteracted by the opposite rotation produced by the other.*

To get complete neutralisation, it is necessary that the element should be of the same size, and that the two varieties should be twisted (in opposite directions) to the same amount. The experiment was repeated in the following order, to avoid any uncertainty due to the possible variation of the sensitiveness of the receiver. The receiver is adjusted to a particular sensitiveness, and as long as it is not disturbed by the action of radiation, the sensitiveness remains constant. A mixture of opposite elements is first interposed,

* Preston, 'On Light,' 2nd ed., p. 421.

the receiver continuing to remain unaffected. From the mixture of positive and negative varieties, one set, say the negative, is now rapidly withdrawn, and an equal number of positive substituted. The receiver which has not been disturbed since its first adjustment is now found to respond, all the elements conspiring to produce rotation in the same direction. It will be seen that the two experiments are carried out under identical conditions.

In the above, we have electro-optic analogues of two varieties of sugar—dextrose and levulose. There is also the production of an apparently inactive variety by the mixture of two active ones.

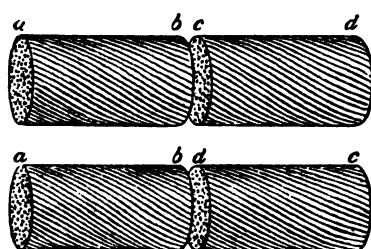


FIG. 2.—Jute elements.

It is to be noted that there is no polarity in the elements, in the sense we use the term in reference to, say, magnetic molecules. *There is nothing to distinguish one end of the jute element from the other end; indeed a right-handed element would appear right-handed when looked at from either end.* It thus happens that if the rotation is determined by the direction of the twist, two molecules of the same variety will always conspire, whether they are arranged as *ab, cd*, or, to take the extreme case, as *ab, dc* (with the second molecule reversed). The assumption of any particular arrangement of molecules is thus not necessary in explaining the rotation. The average effect produced by a large number of active elements interspersed in an inactive medium will thus be the same in all directions, and proportional to the number of molecules traversed by the ray. *As there is no polarity in the molecule, a right-handed element will always produce the same kind of rotation, say, to the right of an observer travelling with the ray.* The rotation produced when the ray is reversed by reflection will thus be in an opposite direction, and the two rotations will neutralise each other.

But if the molecules exhibit any polarity, that is to say, if the effects produced by the two ends of the same molecule are opposite, the resultant effect produced by a number of such molecules arranged in haphazard directions, will be zero. In order that the effects produced by the molecules may conspire, it is necessary that they should

take up a special arrangement like the disposition of molecules in a magnetised rod. It is seen that in this case the rotations of the direct and the reflected rays are in the same direction, and the resultant rotation is therefore doubled. There is some analogy between the action of such polarised molecules and of substances which, when placed in a magnetic field, rotate the plane of polarisation.

"On the Production of a 'Dark Cross' in the Field of Electromagnetic Radiation." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by Lord RAYLEIGH, F.R.S. Received February 14,—Read March 10, 1898.

A circular piece of chilled glass when interposed between crossed nicols produces a dark cross. A similar effect is produced by crystals like salicine where there is a radial disposition of the principal planes.

I have been able to detect a similar phenomenon in the field of electric radiation by the interposition of an artificial structure between the crossed polariser and analyser.

I have in a previous communication described the polarisation produced by the leaves of a book. For the following experiment, a long strip of paper was rolled into a disc. A roll of Morse's tape serves the purpose very well. The diameter of the disc is 14 cm. and its thickness 2 cm. It will be observed that here we have a single axis passing through the centre, and that all planes passing through the centre are principal planes.

The effect produced by the interposition of the structure may be studied by keeping the disc fixed and exploring the different parts of the field by means of the detector; or the detector may be kept fixed (opposite the analyser) and the disc may be moved about so that the different parts of the field may successively be brought to act on the detector. This latter plan was adopted as being simpler in practice.

The arrangement of the apparatus is the same as in fig. 1 of my paper "On the Rotation of Plane of Polarisation of Electric Waves by a Twisted Structure." The polariser is vertical and the analyser horizontal. The paper disc is interposed between the screens with its plane at right angles to the direction of the ray.

The receiver is fixed on the prolongation of the line (which I shall call *the axis*), joining the centres of the polariser and the analyser.

On the supposition that the interposition of the disc produces a *dark cross*, the arms of the cross (with the particular arrangement

of the polariser and the analyser) will lie in the projections of the vertical and the horizontal diameters of the disc, and will move in space with the movements of the disc. When the centre of the disc is on the axis the intersection of the cross will be superposed on the receiver, and there should then be no action. If the disc be moved up and down, the centre remaining in the vertical line passing through the axis, the vertical arm of the cross will slide over the receiver. If the disc be moved laterally, with its centre in the horizontal line passing through the axis, the horizontal arm of the cross will slide over the receiver. In this, as in the last case, there should be no action on the receiver. But if the disc be displaced so that the centre does not lie in either the horizontal or the vertical line passing through the axis (the axis now cutting the disc at points such as *a*, *b*, *c*, or *d*), the arms of the cross will not fall on the receiver, and there should be a response in the receiver.

The experiments were now arranged as follows:—The disc was at first placed with its centre on the axis, the plane of the disc being

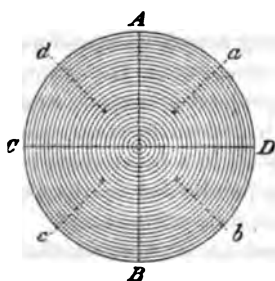


FIG. 1.—The paper disc. AB, CD are the vertical and horizontal diameters.

perpendicular to the axis. There was now no action on the receiver; but as soon as the disc was tilted, however slightly, an action was immediately produced on the receiver.

The disc was now mounted on a stand, between the two screens. By means of sliding arrangements the disc could be raised or lowered, or moved laterally.

In the next experiment, the centre of the disc was first adjusted on the axis, and the disc moved vertically up and down. No effect was produced when this was being done.

The centre of the axis was again adjusted on the axis, and the disc moved laterally on the horizontal slide. In this case, too, there was no action.

By adjusting the vertical sliding rod the centre of the disc was next placed vertically above or below the axis. The disc was then moved laterally either to the right or to the left. In this way the

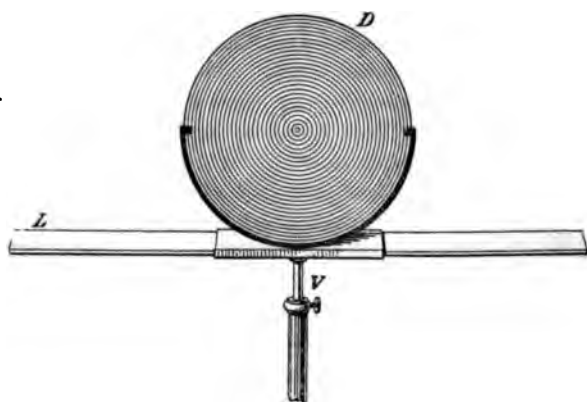


FIG. 2.—The holder for the disc. D, the paper disc. V, L are the vertical and horizontal slides.

field could be displaced, and the quadrants *a*, *b*, *c*, or *d* (see fig. 1) placed opposite to the receiver. In all these cases, even with small displacements, very strong action was produced on the receiver.

From experiments carried out in the manner described above, the outline of a dark cross projected in space was distinctly made out.

A disc of wood, with *concentric rings*, would probably show the effect equally well. I shall in a future paper send an account of the action of crystals cut perpendicular to the axis placed in convergent or divergent beams of electric radiation.

Some of the investigations on the rotation of the plane of polarisation will, perhaps, be facilitated by an observation of the rotation of the cross. By a modification of this method I am at present trying to detect the rotation produced in a magnetic field.

Addendum, 16th March, 1898.

The production of a dark cross can also be demonstrated by interposing between the crossed polariser and analyser concentric rings of tin-foil mounted on a thin sheet of mica. But greater interest is attached to the exhibition of the phenomenon by double refracting substances, where the axes of elasticity are disposed in radial directions. From the peculiar stresses present, I surmised that woody stems with concentric rings would exhibit the phenomenon above described. Through the kindness of Dr. Prain, I obtained from the Government Botanical Gardens, Sibpore, stems of *Pinus longifolia*, *Swietenia mahogani*, *Araucaria Cunninghamii*, *Mangifera indica*, *Casuarina equisetifolia*, *Cupressus torulosa* and *Dalbergia sissoo*. The ring systems present in some of these were very regular,

I was, however, at first disappointed in failing to obtain the results anticipated. But this failure, I subsequently found, was due to the general opacity of the wood which was freshly cut, and which, though apparently dry, contained large quantities of sap in the interior. I then carefully dried some of the specimens, when the stresses present became quite apparent by numerous cracks starting in radial directions. The results obtained with these dried specimens were quite satisfactory.

I now tried to devise some experiments strictly analogous to the optical experiments with chilled glass. For this purpose I cast a cylinder of paraffin wax in a metallic mould surrounded by a freezing mixture. Owing to the great contraction produced by solidification, a hollow depression was formed in the centre, and this produced a distortion of symmetry. It would, therefore, be better to build up a cylinder by successive dippings, the deposited molten layer contracting on the solid core. I obtained, however, extremely good results with a cylinder of cast ebonite, in which the stresses present are exactly similar to those in a circular piece of unannealed glass.

The next series of experiments were undertaken with mineral specimens. I here acknowledge with thanks, the kind help I received from Mr. Hayden, B.A., and Mr. Blyth, of the Geological Department, in obtaining suitable specimens for my experiments. One very interesting specimen obtained from Egypt was formed by ringed concretion of flint round a central nodule. This specimen exhibited the cross with great distinctness. I also obtained fairly satisfactory results with stalactite. The concretion of calcium carbonate formed inside a pipe by deposits from temporarily hard water flowing through it, would also be found to exhibit this phenomenon.

"The Relations between Marine Animal and Vegetable Life."

By H. M. VERNON, M.A., M.B. Communicated by Professor
BURDON SANDERSON, F.R.S. Received December 8, 1897,—
Read January 24, 1898.

(From the Zoological Station, Naples.)

(Abstract.)*

The object of this research was to determine how the nitrogenous matter excreted by marine animals into the water is removed, and what parts the various forms of vegetable life play in the process. Thus this subject is of interest from its practical bearing on questions relating to the efficient maintenance of marine aquaria, as well

* The full paper will be published in the 'Mittheilungen aus der Zoologischen Station zu Neapel.'

as from the theoretical standpoint of the changes taking place under natural conditions in the open sea.

The method of investigation was a triple one, viz., chemical, physiological, and bacteriological. The chemical procedure consisted in carefully determining the free and albuminoid ammonia present in the various specimens of water by means of the well-known method of Wanklyn, Chapman and Smith. In certain cases also the nitrites were determined by the metaphenylenediamine reaction. The water was tested physiologically by allowing the fertilised ova of the sea-urchin, *Strongylocentrotus lividus*, to develop in it, and by determining the change produced in the size of the larvæ after eight days' growth under various conditions. The larvæ were in each case measured under the microscope in groups of fifty. In addition also the proportion of ova arriving at the eight days' larval stage was always determined. The bacterial quality of the water was tested by counting the number of colonies obtained by gelatin plate culture.

With reference to the purifying effects of vegetable life on the water, it was found that green weeds such as *Ulva*, will, when placed in Aquarium tank water, rapidly remove the free ammonia present, though they slowly increase the albuminoid ammonia. Thus in one case 62 per cent. of the free ammonia was removed after two days, and 95 per cent. after ten days, whilst the albuminoid ammonia was increased by respectively 22 per cent. and 27 per cent. If the water be in addition exposed to sunlight, the free ammonia is removed less rapidly than before, but the albuminoid ammonia increased more rapidly. Sea-urchin larvæ grown in water thus purified are, as a rule, increased in size, this increase being in one case 14·4 per cent. Larvæ allowed to develop in direct contact with the alga were, as a rule, found to be slightly diminished in size, although analyses of the water made at the end of the experiments showed that the albuminoid ammonia had been appreciably diminished, as well as the free ammonia. It would seem, therefore, that the alga is able to absorb the albuminoid ammonia when it is present in considerable amount. The proportion of fertilized ova developing to the full larval stage was considerably greater in these experiments than when no alga had been added to the water, hence the effect of the weed would seem on the whole to have been a favourable one.

Red weeds, such as *Gelidium*, when kept in Aquarium water, cause a considerable increase in the albuminoid ammonia, and as a rule also increase the free ammonia. Larvæ grown in water thus treated are diminished in size, but when allowed to develop in direct contact with smaller quantities of the weed, may be increased in size. Small quantities of red and green weeds combined cause an increased number of the ova to reach the larval stage, and may also cause an increase in the size of the larvæ.

On filtration of water through sand which had been taken from one of the tanks in the Aquarium, it was found that 94 per cent. of the free, and 18 per cent. of the albuminoid, ammonia had been removed. This great purification was due to the thin layer of algæ and diatoms with which the grains of sand were covered, for on heating some of this sand to 70° C., and subsequently washing it to remove the vegetable *débris*, the ammonia in the water filtered through it was considerably increased in amount. Also on filtering water continuously for some weeks through a layer of sand 16 cm. deep, which contained small quantities of vegetable matter, a half the free and an eighth of the albuminoid ammonia were at first removed. After a week, however, 58 per cent. of the free and 21 per cent. of the albuminoid ammonia were absorbed, and after a fortnight respectively 96 per cent. and 51 per cent. This marked increase of purification was due to the deposition in the sand of the small quantities of algæ and diatoms in suspension in the water, and to their subsequent multiplication.

This vegetable filter was found to act most efficiently with the water flowing through at its maximum rate. This, in the present experiment, was at 1 litre in three minutes through a superficial area of sand of 177 sq. cm. When, by clamping the exit tube, the rate of flow was diminished to a litre in fifty minutes or more, there was no longer any purification, but the amount of free ammonia in the water was increased some threefold, whilst the albuminoid ammonia remained practically unaltered. Also, on discontinuing the current of water through the filter for twenty-four hours and then renewing it, its purifying efficiency was greatly diminished in respect of the albuminoid ammonia, only 9 per cent. of this being now absorbed. In the course of a few days, however, the purifying power began to increase again. The filter was then covered up so as to totally exclude the light, and thereby kill off the chlorophyll-containing organisms. About 90 per cent. of the free ammonia was still absorbed, but never more than 20 per cent. of the albuminoid ammonia, even after twenty-five days more of continuous filtration. This purification must have been due to bacterial agency. Such a "bacterial" sand filter reacts to changes in the rate of flow of the water quite differently to the chlorophyll-containing one. Thus the purification remained practically as great on diminishing the rate of flow to 1 litre in two hours fifty minutes, and the water flowing through immediately after the current had been entirely stopped for ten hours had 93 per cent. of its free ammonia removed.

In another experiment, sand which at first contained no vegetable matter at all was found to have practically no purifying power. On allowing a current of water to flow through it continuously, however, this gradually developed and increased in amount as in the above experiment.

Larvæ grown in water purified by this filtration through sand are increased in size some 4·2 per cent., whilst as a rule the percentage of ova reaching the full larval stage is considerably increased.

On keeping sea water in diffuse light the free ammonia soon begins to disappear, owing to the multiplication of the small quantities of algæ and diatoms in suspension in the water. Thus, in one instance, after ten days 40 per cent. of the free ammonia had disappeared, and after twenty-five days 92 per cent.; the albuminoid ammonia had, on the other hand, increased by respectively 28 and 101 per cent. Larvæ grown in water thus purified were on an average increased in size by 13·4 per cent.

On keeping water in direct sunlight for some days, the free ammonia is somewhat diminished, and the albuminoid ammonia considerably increased, probably owing to the formation of vegetable growth. Larvæ grown in water which had been exposed four or more days to the sun and air in a covered glass jar were on an average unaffected in size, though only 11 per cent. of the fertilized ova employed reached the larval stage. On the other hand, with water exposed four or five days to the sun in a flask, whereby very little surface was open to the air, nearly the normal number (i.e., 66·6 per cent.) of ova developed to larvæ, and these larvæ were no less than 16·7 per cent. larger than the normal. Bacteriological examination failed to establish any constant differences in the number of germs in these two different kinds of water, but they confirmed the germicidal action of sunlight on the water. It was found, however, that water in which most of the germs had been killed by several days' exposure to sunlight, contained very many more germs than were originally present, if it were subsequently kept in diffuse light. Thus in the subjoined experiment, the water was exposed ten and a half days to direct sunlight, the number of colonies developing after forty-eight hours' incubation being reduced to 70 per c.c. After two days' exposure to diffuse light, however, the number had enormously increased. It will be noticed that the number also increased between the first and fourth days of exposure. This was owing to the fact that there was very little direct sunlight at the time.

Original water	{ 3,500
	{ 2,700
After 1 hour	2,260
„ 3 hours.....	1,360
„ 6½ „	1,450
„ 3½ days	5,500
„ 7½ „	150
„ 10½ „	70
„ 12½ „	320,000

On keeping Aquarium water in darkness for three or more weeks, it undergoes great purification. Thus in one case after twenty-five days there remained only 2 per cent. of the free and 48 per cent. of the albuminoid ammonia originally present. There was no further purification on keeping an additional twenty-nine days. This purification is due to bacterial action, as it is greatly delayed if the water be previously heated to 100°, and is stopped by adding corrosive sublimate to the water. The amounts of free and albuminoid ammonia present after this purification are in some cases no greater than those found in pure open sea water. The physiological purity is not so great, however, as larvæ grown in open sea water are increased in size by 16·0 per cent., and those grown in water kept in darkness by, on an average, 7·5 per cent. Also with this latter water the percentage of ova reaching the full larval stage is slightly smaller than the normal. Water kept in darkness contains considerably fewer germs than normal water, and the longer the water is kept the smaller is the number.

A very considerable purification of the Aquarium water is effected by the layer of bacterial slime coating the inside of the pipes which conduct the water from the reservoirs to the rooms. Thus water drawn off at the rate of a litre in ten seconds contained 26 per cent. less free, and 25 per cent. less albuminoid, ammonia than the reservoir water, and that drawn off at a litre in two hours and forty minutes respectively 82 per cent. and 16 per cent. less. Larvæ grown in such water were, moreover, some 7·8 per cent. larger than the normal.

On filtering water through asbestos, the albuminoid ammonia is somewhat increased, but larvæ grown in the filtered water are some 12·6 per cent. larger than the normal.

Larvæ grown in water previously heated to 50°, 76°, or 100° are increased in size by some 6·2 per cent., and the proportion of ova reaching the larval stage is also increased. This is probably due to the removal of bacteria and other forms of life from the water, though after the first day or two the water contains more bacteria than unheated water.

Ordinary tank water gave about 1500 colonies per c.c. after twenty-four hours' incubation at 25°, and about 3000 after forty-eight hours. Filtration through sand at first diminished the number of germs, but when this became impregnated with vegetable growth they were largely increased. Through sand kept in darkness, very slow filtration increases the number of germs, but a fairly fast one diminishes them. The addition of algæ to the water increases the number of germs.

Larvæ grown in water previously fouled by fish, crabs, molluscs, and Holothurians are *increased* in size on an average by 4·1 per cent., but with water fouled by sea-anemones and medusæ are slightly

diminished in size. The increase in size appears to depend roughly on the amount of contamination of the water. Fish and crabs appear to effect about ten times as much contamination as molluscs and Holothurians.

On the other hand, if the water be previously fouled by the sea-urchins *Stronglyocentrotus*, *Sphærechinus*, and *Echinus*, a decrease amounting on an average to 6·9 per cent. is effected; but with *Arbacia* and *Dorocidaris* there is on an average an increase of 3 per cent. There is indeed considerable evidence to show that an organism exerts a special adverse influence on the members of its own species. The fouling products do not seem to be of the nature of ptomaines, as larvæ grown in water fouled by dead sea-urchins were in some cases increased in size, though on an average they were slightly diminished. These dead sea-urchins were found to effect about ten times as much contamination of the water as did the various living animals examined. In connection with these results, reference may be made to some previous experiments,* in which it was found that the addition of small quantities of uric acid and urea to the water caused an increase in the size of the larvæ, this increase amounting in one instance to 12·2 per cent. Also it was found that the addition of considerable quantities of CO₂ to the water excited a positive rather than a negative effect on the size of the larvæ.

Larvæ grown in water in which another batch of larvæ had already developed were on an average diminished by 6·9 per cent.

The evidence as to whether a pluteus reacts more to its own products of metabolism than to those of other species of plutei was too variable to afford a definite conclusion either way. With water fouled by plutei, the proportion of fertilised ova reaching the full larval stage is as a rule considerably less than the normal; with that fouled by living Echinoids it is slightly less; with that fouled by various other animals about the same, and with that fouled by dead Echinoids it is greater than the normal.

Ammonium chloride exerts a very fatal action on larval growth. Thus with water containing 1 part in 25,400 of the salt, the larvæ were diminished 19·0 per cent. in size, and only 28 per cent. of the ova reached the larval stage. Potassium nitrite, if kept below about 0·3 gram per litre, and potassium nitrate, if kept below 1 gram, have practically no action.

Larvæ grown in sea water which had been aerated by prolonged agitation were on an average increased by 0·65 per cent. The bacterial quality of the water seemed practically unaffected by the aëration.

After keeping water several weeks in darkness the nitrites originally present are much diminished, or disappear altogether. After

* 'Phil. Trans.,' B (1895), p. 595.

filtration through sand impregnated with vegetable growth, and after adding algæ to the water, the nitrites are increased. The growth of larvæ in a water is as a rule accompanied by an increase of nitrites, though the nitrification does not seem to be increased on the addition of either ammonium salts or nitrates to the water, or on fouling the water by animal excretions.

The arm-lengths of the larvæ are not specially affected by vegetable growth, though by water filtered through sand impregnated with algæ and diatoms they are somewhat diminished. They are considerably increased on development of the larvæ in water purified by being kept in darkness, and in aerated water. They are greatly diminished in water previously heated to 100°, but not in that heated to from 50° to 77°. In water exposed to sunlight they are also diminished. They are increased in water fouled by most animals and by dead Echinoids, but in that fouled by living Echinoids are diminished.

During a period of seven months the specific gravity of the Aquarium water was found to vary from 1·02859 to 1·02964 at 15·56° C. The specific gravity was on an average about 0·00040 greater than that of the open sea water. The free ammonia varied from 0·185 to 0·350 milligram per litre, and the albuminoid from 0·111 to 0·182 milligram.

*Report of the Kew Observatory Committee for the Year
ending December 31, 1897.*

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Observatory Committee, which is constituted as follows :—

Mr. F. Galton, Chairman.

Captain W. de W. Abney, C.B., R.E.	Prof. A. W. Rücker.
Prof. W. G. Adams.	Mr. R. H. Scott.
Captain E. W. Creak, R.N.	Mr. W. N. Shaw.
Prof. G. C. Foster.	Lieut.-General Sir R. Strachey, G.C.S.I.
Prof. J. Perry.	Rear Admiral Sir W. J. L. Wharton, K.C.B.
The Earl of Rosse, K.P.	

The work at the Observatory may be considered under the following heads:—

- 1st. Magnetic observations.
- 2nd. Meteorological observations.
- 3rd. Solar observations.
- 4th. Experiments and Researches in connexion with any of the departments.
- 5th. Verification of instruments.
- 6th. Rating of Watches and Marine Chronometers.
- 7th. Miscellaneous.

I. MAGNETIC OBSERVATIONS.

The Magnetographs have been in constant operation throughout the year, and the usual determinations of the Scale Values were made in January.

The ordinates of the various photographic curves representing Declination, Horizontal Force, and Vertical Force were then found to be as follows:—

Declinometer: 1 inch = $0^{\circ} 22' 04''$. 1 cm. = $0^{\circ} 8' 7''$.

Bifilar, January 20, 1897, for 1 inch $\delta H = 0.0280$ foot grain unit.

„ 1 cm. „ = 0.00051 C.G.S. unit.

Balance, January 21, 1897, for 1 inch $\delta V = 0.0274$ foot grain unit.

„ 1 cm. „ = 0.00050 C.G.S. unit.

During the past year the magnetic curves have been free from any very large fluctuations. The principal variations that were recorded took place on the following days:—

January 2; February 4, 25—27; March 9—10; April 2, 20, 23—24; May 17; June 16—17; July 31; October 1—2, 28; November 17; December 11, 20—21.

The hourly means and diurnal inequalities of the magnetic elements for 1897, for the quiet days selected by the Astronomer Royal, will be found in Appendix I.

In the present year a correction has been applied for the diurnal variation of temperature, use being made of the records from a Richard thermograph as well as the eye observations of a thermometer placed under the Vertical Force shade.

The mean values at the noons preceding and succeeding the selected quiet days are also given, but these of course are not employed in calculating the daily means or inequalities.

The following are the mean results for the entire year:—

Mean Westerly Declination $17^{\circ} 6' 4''$.

Mean Horizontal Force 0.18342 C.G.S. unit.

Mean Inclination $67^{\circ} 19' 6''$.

Mean Vertical Force 0.43906 C.G.S. unit.

Observations of Absolute Declination, Horizontal Intensity, and Inclination have been made weekly, as a rule.

As in 1896, a table of recent values of the Magnetic Elements at the Observatories whose publications are received at Kew was contributed to 'Science Progress,' and appeared in the August number. A similar table, but containing more recent data, will be found in Appendix 1A to the present Report.

In July, M. Moureaux, of the Parc Saint-Maur Observatory, near Paris, paid a visit, and a comparison was made of his and the Kew magnetic instruments, a detailed report of which has been drawn up by the Superintendent, and published in the Royal Society's Proceedings, vol. 62, p. 156.

The magnetic instruments lent to the Jackson-Harmsworth Polar Expedition have been returned, and in October some observations were taken with them by Mr. Albert Armitage, the Magnetic Observer in the expedition, and the Observatory Staff, with a view to standardizing the Arctic Observations.

Dr. van Rijkevorsel spent some time at the Observatory, in March and April, comparing his magnetic instruments with the Kew unifilar and dip circle.

Information on matters relating to various magnetic data have been supplied to Dr. E. Atkinson, Professor Arnold Lupton, and Captain Schück, and the latter gentleman compared his instruments with the Observatory standards.

II. METEOROLOGICAL OBSERVATIONS.

The several self-recording instruments for the continuous registration of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year, and the standard eye observations for the control of the automatic records duly registered.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the Institute of Mining Engineers, and the editor of 'Symons' Monthly Meteorological Magazine.'

On June 21, observations with the Campbell sunshine recorder, of the original wooden bowl pattern, were suspended, by direction of the Meteorological Council.

Electrograph.—The auxiliary battery of 60 chloride of silver cells

used with this instrument was received back from the makers on January 11.

Before restarting the instrument, the Clifton Quadrant Electrometer was taken entirely to pieces, all parts thoroughly cleaned and dried, and new sulphuric acid put in the inner jar.

The battery was tested, each row of cells being examined and the voltage determined.

The electrograph was started on January 19, and has been in constant operation since, with the exception of one or two short stoppages due to freezing of the water jet, or other accidental causes. Owing presumably to the changes introduced last year, there has been a great improvement in the behaviour of the apparatus. There are still, however, one or two directions in which further improvement is desirable.

On September 29, one-third of the cells in the battery were taken off, to make a corresponding contraction in the scale values, which was expedient in view of the high potentials usually recorded during the winter months.

Notwithstanding this, several hours' record have been lost owing to the trace being off the sheet. It is difficult at present to see how such loss can be avoided, without either duplicating part at least of the apparatus, so as to get two curves, one showing ordinary and the other extraordinary potentials (positive and negative), or else by risking possible loss of negative trace by shifting the position, on the sheet, of the zero line.

The scale value was determined, by direct comparison with the Portable Electrometer, White, No. 53, on January 19, May 4, and on September 29, before and after the change above referred to.

The comparisons showed that up to the date of the change the scale value had remained practically constant.

Inspections.—In compliance with the request of the Meteorological Council, the following Observatories and Anemograph Stations have been visited and inspected:—Fleetwood, Stonyhurst, Armagh, Dublin, Valencia, Falmouth, and St. Mary's (Scilly Isles), by Mr. Baker; Radcliffe Observatory (Oxford), Yarmouth, North Shields, Alnwick Castle, Fort William, Glasgow, Aberdeen, and Deerness (Orkney), by Mr. Constable.

III. SOLAR OBSERVATIONS.

Sun-spots.—Sketches of Sun-spots have been made on 165 days, and the groups numbered, after Schwabe's method.

Particulars will be found in Appendix II, Table IV.

Taking into consideration the elaborate photographic work now done elsewhere, the Committee consulted the Solar Physics Com-

mittee and other eminent astronomers on the subject, with the result that they decided that the eye observations should cease at the end of 1897.

IV. EXPERIMENTAL WORK.

Fog and Mist.—The observations of a series of distant objects, referred to in previous Reports, have been continued. A note is taken of the most distant of the selected objects which is visible at each observation hour.

Atmospheric Electricity.—The comparisons of the potential, at the point where the jet from the water-dropper breaks up, and at a fixed station on the observatory lawn, mentioned in last year's Report, have been continued, and the observations have been taken nearly every month.

A comparison of these observations with the corresponding results from the electrograms encourages the belief that there has been no progressive change of insulation in the electrograph, such as was met with prior to the late alterations and improvements.

Advantage was taken of the occurrence of some very thick fogs in November, to carry out six sets of observations of the potential at various heights from the ground to 70 feet above.

Aneroid Barometers.—The experiments referred to last year have been continued, and a considerable number of interesting conclusions have been arrived at. It is hoped that the results will be ready for publication in the course of the present year.

Platinum Thermometry.—In accordance with the arrangement alluded to in last year's Report, Dr. J. A. Harker came to the Observatory in January to do some work in platinum thermometry. The authorities of the International Bureau of Weights and Measures at Sèvres having consented with the greatest readiness and courtesy to a comparison by Dr. Harker, in their laboratories, of the scales of the hydrogen and platinum thermometers, the Committee decided to do all in their power to make the scheme successful. It had been from the first the hope of the Committee that platinum thermometry would prove a valuable auxiliary in direct comparison of mercury thermometers, especially at temperatures outside the range 0° to 100° C., and the opportunity of a comparison with the standard gas thermometer of the Bureau International thus occurred very opportunely.

After Dr. Harker's arrival at Kew it was found that somewhat extensive alterations would be required to fit the existing resistance box for the work at Sèvres, and it also appeared undesirable that the Observatory should be deprived for some months of the means of using platinum thermometers. A new resistance box was accordingly ordered from Messrs. Crompton & Co., embodying the alterations

suggested by the experience of Dr. Harker and the Observatory staff. On its completion, this box was taken to Sèvres by Dr. Harker in July, together with two or three of the platinum thermometers previously in use at the Observatory, and some new ones of higher resistance. Since then Dr. Harker has been engaged on the proposed research in co-operation with Dr. Chappuis of the International Bureau. It is expected that the investigation, so far as practicable at present, will be concluded in a few months.

The inconsistencies in the behaviour of the Callendar-Griffiths resistance box, referred to last year, having been proved to arise principally from the uncertainties of the plugs, it was sent to the makers (the Cambridge Scientific Instrument Company) for readjustment. They made use of the opportunity to introduce, at their own expense, a new system of plugs. In it the plugs are isolated, so that manipulating one leaves the tightness of the others unaffected. Another source of trouble proved to be thermo-electric currents generated in the patent thermo-electric key; the key accordingly has been enclosed, at the suggestion of Mr. W. N. Shaw, in a box, and the defect though still existent appears much reduced.

Experiments have been continued on the fixity of zero of platinum thermometers and the degree of consistency in the results obtained with them. Further attention has also been given to the comparison of platinum and mercury thermometers at high temperatures. This is a subject of increasing urgency in view of repeated requests for direct high temperature verifications which cannot as yet be satisfactorily dealt with.

Mercury Thermometry.—To assist in perfecting a method of high temperature verifications, some high range thermometers of the Jena glasses 16^{III} and 59^{III} have been ordered from Berlin. They are to be verified at the Reichsanstalt before delivery.

At the request of Messrs. Powell & Sons, Whitefriars, experiments are being made as to the thermometric properties of different kinds of glass. Particulars of the chemical composition of the glass will be published, with the results obtained, when the experiments are completed.

An apparatus for the comparison of meteorological maximum and minimum thermometers in the horizontal position has been designed by Mr. Casella, and is at present under construction.

V. VERIFICATION OF INSTRUMENTS.

The subjoined is a list of the instruments examined in the year 1897, with the corresponding results for 1896:—

	Number tested in the year ending December 31.	
	1896.	1897.
Air-meters	5	5
Anemometers	12	3
Aneroids	113	77
Artificial horizons.....	21	17
Barometers, Marine.....	84	167
" Standard	72	101
" Station.....	40	30
Binoculars	455	661
Compasses.....	3	51
Deflectors	—	4
Hydrometers.....	374	292
Inclinometers	8	5
Photographic Lenses	14	10
Magnets.....	4	2
Navy Telescopes	546	707
Rain Gauges.....	17	27
Rain Measuring Glasses.....	26	31
Scales.....	1	—
Sextants.....	591	694
Sunshine Recorders.....	2	10
Theodolites	5	29
Thermometers, Avitreous, or Immisch's	7	5
" Clinical	13,772	17,270
" Deep sea.....	74	119
" High Range	52	37
" Hypsometric	34	30
" Low Range	62	71
" Meteorological	4,098	2,874
" Solar radiation	2	—
" Standard	69	117
Unifilars	3	4
Vertical Force Instruments.....	—	4
Declinometers	—	3
Total.....	<u>20,566</u>	<u>23,457</u>

Duplicate copies of corrections have been supplied in 85 cases.

The number of instruments rejected in 1896 and 1897 on account of excessive error or for other reasons was as follows :—

	1896.	1897.
Thermometers, clinical	161	156
„ ordinary meteorological..	56	38
Sextants	79	98
Telescopes	30	66
Binoculars	—	28
Various	43	56

Two Standard Thermometers have been constructed during the year.

There were at the end of the year in the Observatory undergoing verification 12 Barometers, 680 Thermometers, 24 Sextants, 12 Telescopes, 10 Binoculars, 20 Hydrometers, and 3 Sunshine Recorders.

VI. RATING OF WATCHES AND CHRONOMETERS.

The high standard of excellence to which attention was drawn in last year's Report has been maintained. Although the number of marks obtained by the watch standing first on the list is slightly lower than last year, yet the general average is as good, and no less than 108 movements have obtained the highest possible form of certificate (the class A especially good), which involves the attainment of 80 per cent. of the total marks.

The 680 watches received were entered for trial as below :—

For class A, 492; class B, 144; class C, 16; and 28 for the subsidiary trial. Of these 17 passed the subsidiary test, 161 failed from various causes to gain any certificate; 7 were awarded class C certificates, 109 class B, and 386 class A.

In Appendix III will be found a table giving the results of trial of the first 51 watches which gained the highest number of marks during the year. The highest place was taken by Messrs. Usher and Co^o, of London, with a keyless going-barrel, Karrusel lever-watch, No. 29,106, which obtained 88·4 marks out of a maximum of 100.

The class C trial having been of late years but little called for, the Committee decided early in the year to suspend the further issue of class C certificates, and this rule came into operation on April 1.

The number of watches obtaining the class A certificate “especially good” having during the past few years largely increased, considerations of space forbid the publication of the rates and marks of all of them. Attention was drawn to this proposed change in the Report for 1896.

Appendix III embraces watches gaining 82·5 marks and upwards, the remaining 57, which obtained the distinction “especially good,” ranging from 82·3 to 80·0 marks.

Various representations having been made that changes are

desirable in the system of marks and in the dating of certificates, a circular has been issued to ascertain the general opinion of watch manufacturers and others interested in the matter.

Marine Chronometers.—During the year, 65 chronometers have been entered for the Kew A and B trials; of these 49 gained certificates, and 16 failed to pass.

The present box for the “cold” test of chronometers proving inadequate, it is intended during the winter of 1897–98 to fit up a larger and much improved chamber to hold a considerable number of movements. This, it is expected, will remove much of the difficulty and expense of maintaining the low temperature of 45° during the summer months.

The relay of the chronograph working in circuit with the standard mean-time clock “French” having proved rather uncertain in its action during the latter part of May, it was decided to have the entire apparatus overhauled. This was carried out in June by Messrs. E. Dent & Co., and an improved relay and new armatures were fitted. It has since performed well. During the interval the time signals were recorded on a galvanometer (P.O. pattern).

VII. MISCELLANEOUS.

Paper.—Prepared photographic paper has been supplied to the Observatories at Hong Kong, Mauritius, St. Petersburg, Toronto, Oxford (Radcliffe), and Stonyhurst, and through the Meteorological Office to Aberdeen, Batavia, Fort William, and Valencia.

Anemograph and Sunshine Sheets have also been sent to Hong Kong and Mauritius, and *papier Saxe* to Coimbra, and a number of Campbell-Stokes sunshine recorders to St. Petersburg. At the request of the India Office, a drum chronograph with best frictional governors, to be used for astronomical and transit work, was made by Messrs. Thomas Cook and Sons, of York, and, after erection and examination at Kew, was forwarded to the Colaba Observatory, Bombay.

Exhibition at the Crystal Palace.—Specimens of the curves from the various self-recording apparatus and cloud photographs were lent for exhibition at the Crystal Palace and safely returned.

Ships' Compasses.—With the kind assistance of Captain E. W. Creak, R.N., F.R.S., regulations have been laid down for the separate verification of old or additional cards for compass bowls previously tested.

Travellers' Azimuth Compasses.—After the conclusion of the special experiments mentioned last year, a report was submitted to the Royal Geographical Society.

Painting of the Building.—The whole exterior of the building was painted in April and May by H.M. Office of Works.

Seismograph.—An application for a grant of £60 for the purchase

and erection of a seismograph was favourably entertained by the Government Grant Committee. The instrument has been ordered from Mr. R. Munro, but has not yet been delivered.

National Physical Laboratory Committee.—A committee having been appointed by Government “to consider and report upon the desirability of establishing a National Physical Laboratory for the testing and verification of instruments for physical investigation, for the construction and preservation of standards of measurement, and for the systematic determination of physical constant and numerical data useful for scientific and industrial purposes; and to report whether the work of such an institution, if established, could be associated with any testing or standardising work already performed wholly or partly at the public cost,” the Chairman and the Superintendent gave evidence before it.

It is also proposed that the National Physical Laboratory Committee should visit Kew Observatory at an early date.

Library.—During the year the library has received publications from

21 Scientific Societies and Institutions of Great Britain and Ireland,

102 Foreign and Colonial Scientific Establishments, as well as from several private individuals.

The card catalogue has been proceeded with.

Audit, &c.—The accounts for 1897 have been audited by Mr. W. B. Keen, Chartered Accountant, on behalf of the Royal Society, and by Professor Perry on behalf of the Committee.

The balance sheet, with a comparison of the expenditure for the two years, 1896 and 1897, is appended.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

C. Chree, Sc.D., F.R.S., Superintendent.

T. W. Baker, Chief Assistant.

E. G. Constable, Observations and Rating.

W. Hugo, Verification Department.

J. Foster ” ”

T. Gunter ” ”

W. J. Boxall ” ”

G. E. Bailey, Accounts and Library.

E. Boxall, Observations and Rating, and seven other Assistants.

A Caretaker and Housekeeper are also employed.

(Signed) W. GRYLLE ADAMS,

pro Chairman.

March 29, 1898.

Comparison of Expenditure during the Years 1896 and 1897.

Expenditure.	1896.	1897.	Increase.	Decrease.
	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Administration :—				
Superintendent.....	500 0 0	500 0 0		
First Assistant.....	273 18 0	331 18 0	58 0 0	
Office.....	105 12 1	119 6 1	13 14 0	
Rent, Fuel, Lighting, &c.	88 13 3	88 9 2		0 4 1
Caretaker.....	68 18 0	70 4 6	1 6 6	
Incidental Expenses....	149 18 2	113 2 3		36 15 11
	1186 19 6	1223 0 0	73 0 6	37 0 0
Normal Observatory :—				
Salaries—Observations, &c.....	301 17 8	320 2 10	18 5 2	
Incidental Expenses....	77 1 4	48 1 4		29 0 0
Prop. Adm. Expenditure	237 0 0	244 12 0	7 12 0	
Researches :—				
Salaries.....		110 0 0	110 0 0	
Purchase of Apparatus, &c.....	153 6 1	209 11 1	56 5 0	
Prop. Adm. Expenditure	355 10 0	366 18 0	11 8 0	
Tests :—				
Salaries.....	812 3 6	898 11 6	86 8 0	
Incidental Expenses....	189 14 11	203 0 6	13 5 7	
Prop. Adm. Expenditure	475 19 6	489 4 0	13 4 6	
Commissions :—				
Purchases for Colonial Institutions, &c.	185 6 3	398 18 2	213 11 11	
Prop. Adm. Expenditure	118 10 0	122 6 0	3 16 0	
Purchase of Stock	471 1 0			471 1 0
Gross Expenditure.... (showing an increase of £33 15s. 2d.).	3377 10 3	3411 5 5	533 16 2	500 1 0
Extraordinary Expenditure.				
Researches :—				
Salaries.....		110 0 0	110 0 0	
Purchase of Apparatus, &c.	150 4 2	206 0 7	55 16 5	
Commissions :—				
Purchases for Colonial Institutions, &c.....	185 6 3	398 18 2	213 11 11	
Purchase of Stock	471 1 0			471 1 0
	806 11 5	714 18 9	379 8 4	471 1 0
Leaving for Ordinary Nett Expenditure..... (showing an increase of £125 7s. 10d.).	2570 18 10	2696 6 8	154 7 10	29 0 0

Report of the Kew Observatory Committee.

Kew Observatory. Account of Receipts and Payments for the year ending December 31st, 1897.

RECEIPTS.		PAYMENTS.	
	£ s. d.		£ s. d.
To Balance from Year 1896	130 1 2	By Normal Observatory:—	
Royal Society:—		Salaries—Observations, Tabulations, &c.	320 2 10
Gasolot Trust	444 1 4	Incidental Expenses, Photographic Paper, &c.	48 1 4
" " Income Tax returned.....	14 19 3	Proportion of Administration Expenditure	244 12 0
			612 16 2
Meteorological Council:—		Researches:—	
Allowance.....	400 0 0	Salaries	110 0 0
Postages, &c.	13 11 11	Purchase of Apparatus, &c.	209 11 1
		Proportion of Administration Expenditure	866 18 0
			686 9 1
Tests:—		Tests:—	
Verification	1695 6 10	Salaries	888 11 6
Baling	611 5 11	Incidental Expenses—Apparatus, &c.	203 0 6
Lenses	2 9 1	Proportion of Administration Expenditure	489 4 0
	2309 1 10		1680 16 0
Researches:—		Commissions:—	
Government Grant Committee for purchase of Seismograph	60 0 0	Purchase of Instruments and Photographic Paper	
Commissions executed for Colonial and Foreign Institutions, &c. ...	427 15 4	for Colonial and Foreign Institutions, &c.	398 18 2
Bents	4 13 0	Proportion of Administration Expenditure	122 6 0
Dividends on India Stock	43 19 8		896 1 7
		Balance—in London and County Bank	20 8 6
		Awaiting Banking	20 8 0
		In hand (Petty Cash)	436 18 1
	£3843 3 6		£3848 3 6

ADMINISTRATION EXPENDITURE.

Particulars.	£ s. d.	Apportionment.	£ s. d.
Superintendent.....	500 0 0	Observatory	244 12 0
First Assistant, Librarian, &c.	481 4 1	Researches	266 18 0
Bent, Fuel, &c.	88 9 2	Tests	159 4 0
Caretaker, Repairs, &c.	183 6 9	Commissions	122 6 0
	41223 0 0		41223 0 0

Audited on behalf of the Royal Society and found correct,
18th of January, 1898.

(Signed) W. B. KEEN, Chartered Accountant.

On behalf of the Committee, supervised and approved,
19th January, 1898.

(Signed) JOHN PERRY.

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement				436	13	1
£1800 India 3½ per cent. Stock, value on January 1, 1898.....				1821	0	0
Payments due:—						
Meteorological Council—Allowance, Postages, &c.	100	9	10			
Test Fees.....	608	13	8			
Commissions	168	6	0			
				875	9	6
Stock:—						
Blank Forms and Certificates	61	14	5			
Standard Thermometers	76	12	0			
				138	6	5
				<u>£2971</u>	<u>14</u>	<u>0</u>

January 1898.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Rent, Repairs, &c.....	31	1	4
Observatory accounts—Photographic Paper, &c.	7	14	6
Tests accounts—Repairs, Apparatus, &c.	44	18	4
Commissions	124	3	3
Researches	14	17	11
Government Grant Committee for Seismograph.....	60	0	0
General Balance	2688	18	8

(Signed) CHARLES CHREE,
Superintendent.

£2971 14 0

List of Instruments, Apparatus, &c., the Property of the Kew Observatory Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	Articles specified in the list in the Annual Report for 1893.	1876
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
	Pair 9-inch Dip-Needles with Bar Magnets ...	1887
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
Radcliffe Observatory, Oxford.	Black Bulb Thermometer <i>in vacuo</i>	1897

APPENDIX I.

MAGNETICAL OBSERVATIONS, 1897.

Made at the Kew Observatory, Old Deer Park, Richmond, Lat. $51^{\circ} 28' 6''$ N. and Long. $0^{\text{h}} 1^{\text{m}} 15^{\text{s}} \cdot 1$ W.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinator by Barrow, No. 33, and needles 1 and 2, which are $3\frac{1}{2}$ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1897 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal inequalities, and which have been employed in the preparation of the magnetic tables :—

January	6, 9, 22, 23, 26.
February	2, 9, 17, 18, 20.
March	14, 15, 16, 18, 20.
April.....	3, 11, 12, 15, 22.
May	8, 9, 12, 16, 28.
June.....	8, 9, 10, 12, 30.
July	1, 9, 13, 18, 26.
August.....	4, 5, 6, 24, 31.
September	13, 18, 19, 26, 28.
October.....	5, 9, 13, 20, 21.
November	7, 8, 12, 23, 30.
December.....	8, 13, 26, 27, 28.

Table I.—Hourly Means of the Declination, as determined from the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
(17° +) West. Winter.													
1897.	'	'	'	'	'	'	'	'	'	'	'	'	'
Months.	'	'	'	'	'	'	'	'	'	'	'	'	'
Jan. ..	11·4	7·9	8·0	8·2	8·4	8·3	8·2	8·2	8·0	7·8	7·8	8·9	10·2
Feb. ..	11·6	8·3	8·6	8·6	9·0	8·9	8·8	8·4	8·0	8·0	7·8	7·8	10·1
March.	11·6	6·8	6·9	7·2	7·1	7·0	6·8	6·9	6·1	4·7	4·7	6·2	8·7
Oct. ..	8·9	3·9	3·8	3·9	4·2	3·9	3·7	3·3	2·5	1·9	2·1	3·6	6·6
Nov. ..	7·2	3·0	3·2	3·7	3·9	4·3	3·8	3·7	3·7	3·6	3·6	4·5	5·9
Dec. ..	4·7	2·4	2·8	3·3	3·3	3·3	3·4	3·2	3·1	3·1	2·9	3·3	3·8
Means	9·2	5·4	5·6	5·8	6·0	6·0	5·8	5·6	5·2	4·9	4·8	5·7	7·6
Summer.													
April..	13·3	7·6	7·4	7·5	7·2	6·9	7·0	6·1	4·8	3·6	3·8	5·4	8·1
May ..	12·3	7·6	7·4	7·1	6·8	6·4	5·5	4·5	3·5	3·0	4·2	6·3	9·5
June ..	10·4	5·9	5·9	5·5	5·2	4·7	3·1	2·3	2·3	2·4	3·4	6·0	8·0
July ..	9·8	5·1	4·8	4·7	4·2	3·5	2·3	1·3	1·8	2·2	2·8	4·9	7·7
Aug. ..	9·8	4·2	4·3	4·3	4·1	3·8	3·0	2·2	1·6	2·0	2·9	5·1	7·7
Sept...	9·5	3·4	3·2	3·3	3·2	2·7	3·6	3·2	2·7	2·2	3·1	4·5	6·5
Means	10·8	5·6	5·5	5·4	5·1	4·7	4·1	3·3	2·8	2·6	3·4	5·4	7·9

Table II.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer Means.												
	-0·7	-0·8	-0·9	-1·2	-1·7	-2·3	-3·1	-3·6	-3·8	-3·0	-1·0	+1·6
Winter Means.												
	-1·0	-0·8	-0·6	-0·4	-0·4	-0·6	-0·8	-1·2	-1·5	-1·6	-0·7	+1·2
Annual Means.												
	-0·9	-0·8	-0·8	-0·8	-1·1	-1·4	-1·9	-2·4	-2·7	-2·3	-0·8	+1·4

NOTE.—When the sign is + the magnet

d quiet Days in 1897. (The Mean for the Year = $17^{\circ} 6' 4''$ West.)

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.												
'	'	'	'	'	'	'	'	'	'	'	'	'
11.5	10.2	9.4	9.5	9.5	9.0	8.5	8.1	7.9	7.6	7.9	8.0	11.8
12.0	11.9	11.3	10.2	10.0	9.6	9.5	9.0	8.5	8.2	8.0	8.1	12.2
13.6	13.9	12.5	10.7	8.9	8.3	7.7	7.8	7.9	7.6	7.1	7.0	12.0
8.1	7.7	6.5	5.2	4.9	4.8	4.2	4.1	3.8	3.4	3.7	3.6	8.5
7.2	6.3	5.6	5.4	5.3	4.6	4.3	4.0	3.2	2.7	2.8	2.8	7.6
4.6	3.8	3.8	3.7	3.3	3.0	2.8	2.6	2.5	2.2	2.5	2.5	4.6
9.5	9.0	8.2	7.5	7.0	6.6	6.2	5.9	5.6	5.3	5.3	5.3	9.5
Summer.												
'	'	'	'	'	'	'	'	'	'	'	'	'
13.7	13.5	12.0	10.5	9.1	8.1	7.8	8.1	8.2	8.1	7.9	7.5	12.5
12.5	11.9	10.0	8.6	7.7	7.4	7.4	7.4	7.4	7.4	7.1	7.1	11.9
10.5	10.5	9.9	8.7	7.8	7.4	7.0	6.8	6.8	6.6	5.8	5.5	10.9
10.6	10.4	9.7	8.1	6.7	5.6	6.0	5.9	5.6	5.8	5.3	5.1	10.4
11.5	11.2	9.6	7.7	6.2	5.2	4.9	5.0	5.1	4.9	4.8	4.6	10.4
9.1	8.9	8.2	7.0	6.4	6.4	5.7	4.9	4.7	4.4	4.4	3.8	9.1
11.3	11.1	9.9	8.4	7.3	6.7	6.5	6.4	6.3	6.2	5.9	5.6	10.9

tion as deduced from Table I.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Means.											
'	'	'	'	'	'	'	'	'	'	'	'
+5.0	+4.7	+3.6	+2.1	+1.0	+0.4	+0.1	0.0	0.0	-0.1	-0.5	-0.7
Winter Means.											
'	'	'	'	'	'	'	'	'	'	'	'
+3.1	+2.6	+1.8	+1.1	+0.6	+0.2	-0.2	-0.5	-0.8	-1.1	-1.1	-1.1
Annual Means.											
'	'	'	'	'	'	'	'	'	'	'	'
+4.1	+3.7	+2.7	+1.6	+0.8	+0.3	0.0	-0.2	-0.4	-0.6	-0.8	-0.9

the west of its mean position.

Table III.—Hourly Means of the Horizontal Force in C.G.S. units (corrected
(The Mean for the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
0.18000 + Winter.													
1897. Months.													
Jan. ...	320	326	327	327	329	331	332	333	333	328	324	319	317
Feb. ...	324	335	335	334	334	335	336	335	336	334	330	326	325
March...	319	337	337	337	337	338	339	340	340	335	326	320	319
Oct. ...	332	350	348	347	347	349	349	349	346	340	333	328	330
Nov. ...	339	345	345	344	347	349	351	351	349	348	343	337	338
Dec. ...	346	347	348	347	351	353	355	355	354	355	354	349	346
Means..	330	340	340	339	341	342	344	344	343	340	335	330	329
Summer.													
April...	300	331	331	331	329	331	333	336	331	325	316	307	302
May ...	331	345	345	344	345	344	343	340	334	328	324	322	325
June ...	332	350	349	348	348	348	345	341	337	333	329	331	331
July ...	329	354	354	352	351	351	349	346	341	334	329	323	325
Aug. ...	331	348	348	347	347	346	345	342	338	330	327	327	330
Sept. ...	341	358	358	358	357	357	357	356	353	346	339	336	340
Means..	327	348	347	347	346	346	345	343	339	333	327	324	325

Table IV.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer Means.												
	+ '00004	+ '00003	+ '00003	+ '00002	+ '00002	+ '00001	- '00001	- '00005	- '00011	- '00017	- '00020	- '00019
Winter Means.												
	- '00000	- '00000	- '00001	+ '00001	+ '00002	+ '00004	+ '00004	+ '00003	- '00000	- '00005	- '00010	- '00011
Annual Means.												
	+ '00002	+ '00002	+ '00001	+ '00001	+ '00002	+ '00002	+ '00002	- '00001	- '00006	- '00011	- '00015	- '00015

NOTE.—When the sign is + the

for Temperature) as determined from the selected quiet Days in 1897.
Year = 0·18342.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.													
319	325	329	328	328	330	331	332	331	331	328	328	327	321
329	333	333	333	333	334	334	335	335	337	338	338	337	321
323	327	329	331	334	337	337	341	343	343	341	342	340	323
337	344	346	347	346	348	350	354	353	354	352	354	353	339
340	343	344	342	344	349	350	349	349	348	348	350	348	343
350	354	354	354	353	354	354	354	354	354	353	353	352	351
333	338	339	339	340	342	343	344	344	344	343	344	343	333
Summer.													
307	312	320	327	333	337	338	342	340	340	341	342	341	310
330	340	343	348	348	352	355	357	355	354	350	353	350	331
333	341	341	344	346	351	354	360	359	359	357	356	353	340
332	340	346	354	354	359	361	362	363	363	360	359	356	339
334	336	340	344	348	352	351	357	355	355	352	351	351	342
347	351	355	357	357	358	359	363	364	363	364	362	362	342
331	337	341	346	348	351	353	357	356	356	355	354	352	334

Horizontal Force as deduced from Table III.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Means.												
-·00013	-·00007	-·00003	+·00002	+·00004	+·00007	+·00009	+·00013	+·00012	+·00012	+·00011	+·00010	+·00008
Winter Means.												
-·00007	-·00002	-·00001	-·00001	·00000	+·00002	+·00003	+·00004	+·00004	+·00004	+·00003	+·00004	+·00003
Annual Means.												
-·00010	-·00008	-·00002	-·00000	+·00002	+·00005	+·00006	+·00008	+·00008	+·00008	+·00008	+·00007	+·00008

reading is above the mean.

Table V.—Hourly Means of the Vertical Force in C.G.S. units (corrected)
(The Mean for the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
0.43000 + Winter.													
1897. Months.													
Jan. ...	896	911	912	912	912	913	912	912	913	913	911	909	909
Feb. ...	928	925	925	926	926	926	926	925	925	924	923	920	918
March ...	937	948	948	947	947	946	946	946	946	946	943	938	933
Oct. ...	868	882	882	882	882	881	880	881	882	882	879	875	873
Nov. ...	883	887	886	886	885	885	886	885	885	885	885	884	883
Dec. ...	892	886	886	887	888	888	889	888	888	888	887	887	886
Means	902	906	906	907	907	906	906	906	906	906	905	902	900
Summer.													
April ...	879	897	897	893	894	893	895	895	896	895	889	883	876
May ...	891	917	917	915	915	914	915	914	912	909	906	897	890
June ...	900	920	919	919	920	920	922	922	921	918	914	911	907
July ...	891	908	906	906	905	905	907	905	905	902	899	898	893
Aug. ...	896	914	915	915	914	914	916	917	916	914	907	901	898
Sept. ...	893	904	904	904	903	903	903	903	903	899	896	894	892
Means	892	910	910	909	908	908	910	909	909	906	902	897	893

Table VI.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer Means.												
	+ '00004	+ '00003	+ '00002	+ '00002	+ '00002	+ '00003	+ '00003	+ '00002	'00000	- '00006	- '00009	- '00014
Winter Means.												
	'00000	'00000	'00000	'00000	'00000	'00000	'00000	'00000	'00000	- '00002	- '00004	- '00006
Annual Means.												
	+ '00002	+ '00002	+ '00001	+ '00001	+ '00001	+ '00002	+ '00001	+ '00001	'00000	- '00003	- '00007	- '00010

NOTE.—When the sign is + the

temperature), as determined from the selected quiet Days in 1897.
 $\alpha = 0.43906$.)

1.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.													
	911	916	916	915	915	915	915	914	914	913	913	912	918
	923	929	932	933	932	931	929	928	926	925	923	924	912
	933	939	946	950	952	951	951	950	950	949	949	940	926
	878	880	883	884	883	883	883	883	883	882	880	879	875
	887	890	892	892	891	889	889	889	890	888	888	888	879
	887	883	888	888	888	888	887	887	886	887	887	887	875
	903	907	909	910	910	909	909	908	908	907	906	906	898
Summer.													
	877	883	890	893	895	895	897	896	895	894	895	894	872
	897	904	910	912	917	918	919	917	917	917	917	914	888
	911	913	918	922	923	924	925	927	926	924	924	922	903
	895	901	907	913	915	916	915	914	913	911	910	908	887
	898	903	911	914	915	917	915	914	913	911	910	910	896
	894	896	901	905	904	905	905	905	905	905	904	904	897
	895	900	906	910	911	912	913	912	911	910	910	909	891

ical Force as deduced from Table V.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Means.											
14	-00011	-00006	00000	+00003	+00005	+00006	+00008	+00008	+00008	+00004	+00002
Winter Means.											
25	-00003	00000	+00003	+00004	+00004	+00003	+00003	+00002	+00002	+00001	00000
Annual Means.											
19	-00007	-00003	+00001	+00003	+00004	+00004	+00004	+00004	+00003	+00002	+00002

; is above the mean.

Table VII.—Hourly Means of the Inclination, calculated from the Horizons

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
67° + Winter.													
1897. Months.													
Jan....	20·8	20·8	20·8	20·8	20·6	20·5	20·4	20·4	20·4	20·7	21·0	21·2	21·4
Feb....	21·4	20·6	20·6	20·7	20·7	20·6	20·6	20·6	20·6	20·7	20·9	21·1	21·1
March..	22·0	21·1	21·1	21·1	21·1	21·0	20·9	20·9	20·9	21·2	21·7	22·0	21·9
Oct....	19·2	18·4	18·6	18·6	18·6	18·5	18·4	18·5	18·7	19·1	19·5	19·7	19·6
Nov....	19·3	18·9	18·9	18·9	18·7	18·6	18·5	18·4	18·6	18·6	19·0	19·3	19·2
Dec....	19·0	18·7	18·7	18·8	18·5	18·4	18·3	18·3	18·3	18·3	18·3	18·6	18·8
Means..	20·3	19·8	19·8	19·8	19·7	19·6	19·5	19·5	19·6	19·8	20·1	20·3	20·3
Summer.													
April...	21·7	20·1	20·1	20·0	20·1	20·0	19·9	19·7	20·1	20·4	20·9	21·3	21·5
May....	19·9	19·8	19·8	19·7	19·7	19·7	19·8	20·0	20·3	20·6	20·8	20·7	20·3
June...	20·1	19·5	19·5	19·5	19·6	19·6	19·9	20·1	20·4	20·5	20·7	20·5	20·4
July....	20·1	18·9	18·8	19·0	19·0	19·0	19·2	19·3	19·7	20·0	20·3	20·6	20·4
Aug....	20·1	19·4	19·5	19·5	19·5	19·6	19·7	19·9	20·2	20·6	20·7	20·5	20·2
Sept....	19·3	18·5	18·5	18·5	18·5	18·5	18·5	18·6	18·8	19·2	19·5	19·7	19·4
Means..	20·2	19·4	19·4	19·4	19·4	19·4	19·5	19·6	19·9	20·2	20·5	20·6	20·4

Table VIII.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Summer Means.												
	-0·1	-0·1	-0·1	-0·1	-0·1	0·0	+0·1	+0·4	+0·7	+1·0	+1·1	+0·9
Winter Means.												
	0·0	0·0	+0·1	-0·1	-0·2	-0·2	-0·2	-0·2	0·0	+0·3	+0·6	+0·6
Annual Means.												
	-0·1	-0·1	0·0	-0·1	-0·2	-0·1	-0·1	+0·1	+0·4	+0·7	+0·8	+0·7

NOTE.—When the sign is +

Vertical Forces (Tables III and V). (The Mean for the Year = $67^{\circ} 19' 6''$.)

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.												
20.9	20.8	20.8	20.8	20.7	20.6	20.5	20.6	20.6	20.7	20.7	20.8	21.8
20.7	20.9	20.9	21.0	20.9	20.8	20.7	20.7	20.5	20.4	20.4	20.5	21.2
21.4	21.4	21.5	21.4	21.2	21.2	21.0	20.8	20.8	20.9	20.8	21.0	21.5
18.7	18.6	18.6	18.7	18.6	18.4	18.2	18.2	18.2	18.3	18.1	18.1	19.0
19.0	19.0	19.2	19.1	18.7	18.6	18.7	18.7	18.8	18.7	18.6	18.7	18.8
18.3	18.3	18.3	18.3	18.3	18.3	18.3	18.3	18.3	18.4	18.4	18.4	18.2
19.8	19.8	19.9	19.9	19.7	19.6	19.6	19.6	19.5	19.6	19.5	19.6	20.0
Summer.												
20.8	20.4	20.2	19.9	19.6	19.6	19.4	19.5	19.4	19.3	19.3	19.3	20.8
19.5	19.5	19.3	19.4	19.3	19.1	19.0	19.1	19.1	19.4	19.2	19.3	19.8
19.8	19.9	19.8	19.8	19.5	19.3	19.0	19.1	19.1	19.1	19.1	19.3	19.7
19.4	19.2	18.9	19.0	18.7	18.6	18.5	18.5	18.4	18.5	18.6	18.7	19.3
19.8	19.7	19.6	19.4	19.2	19.3	18.9	19.0	19.0	19.1	19.1	19.1	19.3
18.7	18.5	18.5	18.6	18.5	18.5	18.2	18.2	18.2	18.1	18.2	18.2	19.4
19.7	19.5	19.4	19.4	19.1	19.1	18.8	18.9	18.9	18.9	18.9	19.0	19.7

Station as deduced from Table VII.

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Means.											
+0.2	0.0	-0.1	-0.1	-0.4	-0.4	-0.7	-0.6	-0.6	-0.6	-0.6	-0.5
Winter Means.											
+0.1	+0.1	+0.1	+0.1	0.0	-0.1	-0.2	-0.2	-0.2	-0.2	-0.3	-0.2
Annual Means.											
+0.1	+0.1	0.0	0.0	-0.2	-0.3	-0.4	-0.4	-0.4	-0.4	-0.4	-0.3

Reading is above the mean.

APPENDIX IA.

MEAN VALUES, for the years specified, of the Magnetic Elements at Observatories whose Publications are received at Kew Observatory.

Place.	Latitude.	Longitude.	Year.	Declination.	Inclination.	Horizontal Force. C. G. S. Units.	Vertical Force. C. G. S. Units.
Pawlowak	59 41 N.	30 29 E.	1895	0 15.7 E.	70 42.4 N.	16478	47072
Katharinenburg	56 49 N.	60 38 E.	1895	9 43.3 E.	70 39.8 N.	17808	50750
Kasan	55 47 N.	49 8 E.	1892	7 30.8 E.	68 36.2 N.	18551	47345
Copenhagen ...	55 41 N.	12 34 E.	1894	10 41.3 W.	—	17373	—
Stonyhurst	53 51 N.	2 28 W.	1896	18 31.2 W.	68 57.7 N.	17202	44726
Hamburg	53 34 N.	10 8 E.	1896	11 36.7 W.	67 38.8 N.	18061	43921
Wilhelmshaven	53 32 N.	8 9 E.	1896	12 46.8 W.	67 51.7 N.	17994	44229
Potsdam	52 23 N.	13 4 E.	1896	10 14.3 W.	66 38.4 N.	18747	43404
Irkutsk	52 16 N.	104 16 E.	1895	2 6.6 E.	70 11.1 N.	20132	55872
Utrecht	52 5 N.	5 11 E.	1895	14 15.5 W.	67 7.4 N.	18435	43691
Kew	51 28 N.	0 19 W.	1897	17 6.4 W.	67 19.6 N.	18342	43906
Greenwich*....	51 28 N.	0 0	1896	16 56.5 W.	{ 67 10.0 N. 67 9.3 N. }	18367	{ 43622 43598 }
Uccle (Brussels)	50 48 N.	4 21 E.	1896	14 32.5 W.	66 23.5 N.	18925	43300
Falmouth	50 9 N.	5 5 W.	1896	18 47.5 W.	67 5.0 N.	18554	43888
Prague	50 5 N.	14 25 E.	1896	9 25.5 W.	—	19858	—
Parc St. Maur (Paris)	48 49 N.	2 29 E.	1895	15 9.4 W.	65 2.9 N.	19664	42268
Vienna	48 15 N.	16 21 E.	1894	8 43.6 W.	63 12.1 N.	20740	41061
O'Gyalla (Pesth)	47 53 N.	18 12 E.	1895	7 52.5 W.	—	21080	—
Odessa†	46 26 N.	30 46 E.	1896	4 49.6 W.	62 33.9 N.	22038	42452
Pola	44 52 N.	13 51 E.	1896	9 41.7 W.	60 31.8 N.	22061	39042
Nice	43 43 N.	7 16 E.	1897	12 12.8 W.	60 15.4 N.	22318	39069
Toronto‡	43 40 N.	79 30 W.	1896	4 50.1 W.	—	16645	—
Perpignan	42 42 N.	2 53 E.	1895	14 0.4 W.	60 8.5 N.	22363	38958
Rome	41 54 N.	12 27 E.	1891	10 45.1 W.	58 4.6 N.	2324	3730
Tiflis	41 43 N.	44 48 E.	1895	1 48.1 E.	55 47.0 N.	25681	37764
Capodimonte (Naples)	40 52 N.	14 15 E.	1893	9 47.0 W.	56 42.1 N.	—	—
Madrid	40 25 N.	3 40 W.	1895	16 6.6 W.	—	—	—
Coimbra	40 12 N.	8 25 W.	1895	17 42.0 W.	59 43.6 N.	22581	38685
Washington ..	38 55 N.	77 4 W.	1894	3 39.9 W.	70 34.3 N.	19979	56646
Lisbon	38 43 N.	9 9 W.	1895	17 39.1 W.	58 15.7 N.	23344	37731
Zi-ka-wei	31 12 N.	121 26 E.	1895	2 15.6 W.	45 55.1 N.	32679	33743
Hong Kong....	22 18 N.	114 10 E.	1896	0 26.0 E.	31 41.3 N.	36461	22509
Colaba (Bombay)	18 54 N.	72 49 E.	1895	0 36.9 E.	20 48.5 N.	37444	14230
Manila	14 35 N.	120 58 E.	1896	0 51.0 E.	16 39.7 N.	37868	11333
Batavia	6 11 S.	106 49 E.	1896	1 22.0 E.	29 29.5 S.	36768	20795
Mauritius	20 6 S.	57 33 E.	1895	9 55.1 W.	54 37.1 S.	23937	33706
Melbourne	37 50 S.	144 58 E.	1896	8 15.0 E.	67 18.3 S.	23392	55936

* Of the two values of the Inclination and Vertical Force, the first is based on observations with 3-inch dip needles only, the second on combined observations with needles of 3, 6, and 9 inches.

† New magnetic observatory; only four last months' results available in 1896.

‡ Determinations of Inclination and Vertical Force suspended in course of 1896, owing to disturbing action of electric tramway.

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure. Kew Observatory.
1897.

Month.	Thermometer.						Barometer.*						Mean vapour-tension.
	Means of—			Absolute Extremes.			Absolute Extremes.						
	Mean.	Max.	Min.	Max.	Date.	Min.	Date.	Min.	Date.	Max.			
1897.					d. h.		d. h.	ins.	d. h.	ins.	d. h.	in.	
Jan....	35.9	39.3	32.5	45.2	1 2 A.M.	23.8	18 8 & 9 A.M.	29.896	2 10 A.M.	29.213	30 2 P.M.	.181	
Feb....	43.6	47.7	39.5	56.3	26 3 P.M.	28.1	18 3 "	30.113	16 11 "	29.173	2 7 & 8 A.M.	.246	
March..	45.1	51.2	39.2	60.9	23 3 "	30.3	9 6 "	29.698	{ 20 11 " & NOON.	28.778	3 10 "	.238	
April...	46.2	53.1	39.6	66.9	27 5 "	28.3	11 7 "	29.866	15 11 P.M.	28.997	1 7 "	.238	
May ...	51.5	60.6	42.8	72.1	18 NOON.	33.7	13 3 "	29.978	{ 15 11 " & MIDD.	29.343	27 5 P.M.	.265	
June...	61.1	69.6	52.7	84.0	24 1 P.M.	44.6	10 4 "	30.028	16 0.5 A.M.	29.447	18 3 "	.401	
July ...	63.6	72.6	55.0	80.1	24 3 "	43.6	8 4 "	30.021	12 8 "	29.577	20 2 "	.402	
Aug....	62.5	71.5	54.8	87.7	4 2 "	48.0	13 4 "	29.844	8 10 "	29.501	8 2 "	.418	
Sept....	55.2	62.0	48.2	68.3	29 3 "	38.8	19 5 "	30.006	30.531	29.409	1 MIDD.	.347	
Oct.....	50.2	57.4	43.6	65.2	17 1 "	30.0	7 7 "	30.179	21 10 "	29.446	15 11 P.M.	.305	
Nov. ...	45.5	50.1	39.8	58.7	13 NOON.	28.6	26 7 "	30.196	21 11 "	29.162	29 4 A.M.	.268	
Dec. ...	40.7	45.1	35.6	55.1	16 "	21.8 { 25 7 P.M. } 26 2 A.M.		29.957	22 10 "	29.032	10 11 P.M.	.220	
Yearly Means	50.1	56.7	43.6	29.982284	

* Reduced to 32° at M.S.L.

This table has been compiled at the Meteorological Office from values intended for publication in the volume of "Hourly Means" for 1897.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=over-cast).	Rainfall.*			Weather. Number of days on which were registered							Wind.† Number of days on which it was									
		Total.	Maxi- mum.	Date.	Rain. †	Snow.	Hail.	Thun- der- storms.	Clear sky.	Over- cast sky.	Calm.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	
1897.		ins.	ins.																		
January.....	7.5	1.930	0.645	8	18	8	3	19	..	9	5	4	2	3	..	5	3	4	4
February....	8.2	2.185	0.725	4	13	4	20	..	2	2	4	2	2	9	4	3	5	1
March.....	7.4	3.610	0.540	2	19	2	3	1	2	19	5	2	3	..	1	3	10	10	2	1	1
April.....	7.2	1.315	0.285	27	13	1	1	14	..	4	6	3	2	5	5	2	3	2	2
May.....	5.7	1.135	0.280	30	12	1	8	8	..	5	6	..	1	3	8	4	4	1	5
June.....	6.7	2.535	0.815	8	12	2	4	13	..	3	5	4	1	6	4	3	4	4	1
July.....	5.3	0.935	0.625	19	5	2	8	6	..	4	4	3	..	1	8	7	4	6	2
August.....	6.3	2.620	0.640	31	17	1	4	10	3	2	1	4	16	5	..	5	7
September...	7.0	1.955	0.415	1	11	1	2	15	..	3	5	2	..	2	6	7	5	7	9
October.....	5.9	0.580	0.140	25	8	7	11	..	3	5	6	3	5	4	1	4	4	8
November...	7.3	0.805	0.320	27	9	2	19	1	2	7	7	..	2	9	1	2	2	8
December....	6.2	2.140	0.510	7	18	1	5	9	3	3	1	6	1	5	10	4	1	6	6
Totals and means.	6.7	21.835			155	10	3	10	50	163	9	40	52	41	14	41	89	53	35		56

* Measured at 10 A.M. daily by gauge 1.75 feet above ground.

† As registered by the anemograph.

‡ The number of rainy days are those on which 0.01 inch rain or melted snow was recorded.

Meteorological Observations.—Table III.
Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb <i>in vacuo</i> .)			Minimum temperature on the ground.			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date. †	Mean.	Lowest.	Date. ‡	Average hourly velocity.	Greatest hourly velocity.	Date.
1897.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	36 42	14	5 24	26	59	94	25	29	18	18	11.6	34	23
February	42 12	15	8 0	18	73	105	27	35	18	18	10.3	32	25
March	123 42	34	10 24	19	98	118	23	32	16	30	14.5	39	3
April	144 6	34	12 30	15	106	125	29	32	17	11	13.1	34	23
May	260 0	54	14 54	22	119	131	31	36	25	13	11.8	31	21
June	190 30	38	15 24	12	124	143	24	47	34	17	9.5	28	8 { 26 13
July	261 36	53	14 48	15	127	136	3	47	31	8	8.9	32	14 { 17
August	220 42	49	12 54	4	129	141	5	47	37	13	10.8	29	1
September	124 36	33	10 24	3	110	128	25	43	31	19	8.8	33	1 { 16 17
October	99 30	30	8 12	3	95	118	16	37	24	7	8.0	29	29 { 17 29
November.....	26 42	10	5 18	29	67	94	18	35	18	26	9.4	37	29
December.....	44 42	18	5 36	11	62	85	15	30	18	4	13.5	43	29
Totals and Means	1575 0	32	97	37	10.8

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground, the original factor 8 being used.
† Read at 10 A.M., and entered to previous day.
‡ Read at 10 A.M., and entered to same day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1897.			
January	12	7	—
February.....	7	6	—
March.....	19	5	1
April.....	14	6	3
May.....	20	6	5
June	16	2	4
July.....	16	7	—
August	15	6	—
September.....	15	7	—
October.....	12	3	6
November.....	10	4	3
December	9	5	—
Totals for 1897	165	64	22

Table I—continued.

Watch deposited by	Number of watch.	Escapement, balance spring, &c.	Mean daily rate.					Mean variation of daily rate, \pm	Mean change of rate for 10 F.	Difference between extreme gauging and losing rates.	Marks awarded for			Total Marks.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
A. E. Fridlander, Coventry	14393	S.F., g.b., s.o.	-0.1	+2.2	-2.1	+0.2	+3.0	0.4	0.04	6.2	31.9	34.7	17.6	84.2
A. E. Fridlander, Coventry	52856	D.R., g.b., s.o.	+0.3	+0.5	+0.5	+2.2	+3.1	0.5	0.03	4.7	30.5	35.5	18.1	84.1
Newsome & Co., Coventry	126068	S.F., g.b., s.o. "Karrusel"	-1.0	0.5	0.5	+1.7	+0.1	0.5	0.04	5.0	29.5	37.0	17.6	84.1
Newsome & Co., Coventry	130351	S.F., g.b., s.o. "Karrusel"	+0.2	+1.7	+0.7	+3.4	+4.4	0.4	0.03	4.5	32.4	33.6	17.9	83.9
S. Smith & Son, London	20788	S.F., g.b., s.o. "Karrusel"	+2.2	+1.4	+2.3	+3.4	+4.4	0.4	0.05	4.2	31.0	36.4	16.5	83.9
Williamson, Limited, London	54417	S.F., g.b., s.o. "Karrusel"	-1.1	1.3	1.7	+1.0	+0.1	0.4	0.08	5.3	33.0	36.3	14.5	83.8
Newsome & Co., Coventry	129859	S.F., g.b., s.o. "Karrusel"	-2.6	-2.5	-1.8	-2.9	-2.3	0.6	0.04	5.3	27.9	38.8	17.1	83.8
A. E. Fridlander, Coventry	14333	S.F., g.b., s.o. "Karrusel"	-0.1	+3.5	-0.7	+0.1	-2.4	0.4	0.04	7.0	32.0	34.6	17.1	83.7
H. Goly, London	2156	D.R., g.b., d.o., chronograph	1.0	+2.2	-0.7	+0.6	+0.3	0.5	0.03	4.5	29.7	36.0	18.0	83.7
W. Matthews, Coventry	92275	S.F., g.b., s.o. "Karrusel"	1.1	-0.2	+0.6	+2.4	+3.6	0.5	0.07	5.7	32.5	35.6	15.4	83.5
Jos. White & Son, Coventry	35693	S.F., g.b., s.o. "Karrusel"	+1.2	-2.1	-1.9	+0.4	+3.6	0.4	0.01	7.2	31.0	32.8	19.7	83.5
A. E. Fridlander, Coventry	25517	S.F., g.b., s.o. "Karrusel"	+4.3	+4.7	+1.8	+3.0	+4.6	0.4	0.10	7.0	32.3	37.7	13.4	83.4
Rotherham & Sons, Coventry	21777	S.F., g.b., s.o. "Karrusel"	+1.4	+1.7	+1.8	+0.2	+2.7	0.4	0.04	8.0	31.4	34.9	17.1	83.4
Jos. White & Son, Coventry	35762	D.R., g.b., s.o. "Karrusel"	+0.7	+0.4	+1.8	+1.2	+1.6	0.5	0.08	5.8	30.8	34.1	14.5	83.3
Carley & Co., London	50170	S.F., g.b., s.o. "Karrusel"	-2.6	-2.6	-2.8	-2.4	-1.1	0.4	0.08	5.8	31.7	36.6	15.0	83.3
A. E. Fridlander, Coventry	25531	S.F., g.b., s.o. "Karrusel"	-3.1	-3.0	-3.1	-1.6	-1.5	0.6	0.04	4.5	28.6	37.1	17.3	83.2
S. Smith & Son, London	20794	S.F., g.b., s.o. "Karrusel"	+2.0	+3.0	+0.5	-1.0	+1.7	0.6	0.02	6.5	28.5	35.6	19.0	83.1
A. E. Fridlander, Coventry	14356	S.F., g.b., s.o. "Karrusel"	+2.4	+0.4	+0.4	+1.1	+2.2	0.4	0.08	6.3	32.2	35.7	18.0	82.9
D. Buckney, London	30916	D.R., g.b., s.o.	+1.6	+1.4	+0.6	+2.7	+0.6	0.5	0.04	5.7	30.4	35.0	17.4	82.8
Thomas Hill & Co., Coventry ..	2950	S.F., g.b., s.o. "Karrusel"	+0.6	+1.2	+1.3	+0.9	+3.1	0.5	0.06	6.0	29.4	37.3	16.1	82.8
A. E. Fridlander, Coventry	68535	S.F., g.b., s.o. "Karrusel"	-5.8	-4.1	-5.2	-5.7	-0.7	0.5	0.02	7.2	30.1	34.0	18.6	82.7
C. J. H. Marlow, Coventry	25039	S.F., g.b., s.o. "Karrusel"	+1.4	+0.6	+1.5	-0.5	-0.6	0.6	0.01	7.0	28.4	36.8	17.5	82.7
S. Smith & Son, London	02222	D.R., g.b., d.o., "non-magnetic"	-6.4	-5.6	-5.9	-3.4	-5.6	0.6	0.03	9.0	27.7	36.8	18.2	82.7
Jos. White & Son, Coventry	35417	S.F., g.b., s.o. "Karrusel"	-0.6	-1.7	-1.3	-3.2	-3.4	0.4	0.07	7.5	31.6	36.0	18.0	82.6
S. Yeomans, Coventry	76149	S.F., g.b., s.o. "Karrusel"	-0.9	-0.3	-0.9	-0.1	-3.9	0.5	0.02	6.5	29.5	34.2	18.8	82.6
Euame & Co., London	12025	S.F., g.b., s.o. "Karrusel"	+3.8	+3.1	+4.0	+1.1	+3.5	0.6	0.04	10.7	28.2	36.8	17.5	82.6

In the above List, the following abbreviations are used, viz. :—s.f. for single roller; d.r. for double roller; g.b. for going barrel; s.o. for single overcoil; d.o. for double overcoil; + for gaining rate; — for losing rate.

Table II.
Highest Marks obtained by Complicated Watches during the year.

Description of watch.	Number.	Deposited by	Marks awarded for			
			Variation.	Position.	Temperature.	Total marks.
Minute chronograph and minute repeater.....	52944	Fridlander, Coventry	81·0	33·4	14·4	78·8
" "						

"On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helical Current and a Uniform Coaxial Circular Cylindrical Current Sheet." By Professor J. VIRIAMU JONES, F.R.S. Received November 12,—Read December 9, 1897.

§ 1. In measuring electrical resistance by the method of Lorenz we have to determine the coefficient of mutual induction of a helix of wire and the circumference of a rotating circular disc placed coaxially with it, the mean planes of the helix and the disc being coincident. In a paper presented to the Physical Society in November, 1888, I gave a method of calculating this coefficient; but subsequent consideration of the problem in connection with the Lorenz apparatus recently made for the McGill University, Montreal, has led me both to a simplification of the method previously described, and also to a more general solution.

§ 2. If M is the coefficient of mutual induction of any two curves we have

$$M = \iint \frac{\cos \epsilon}{r} ds ds',$$

where r = the distance between two elements ds, ds' ; and ϵ = the angle between these elements. Let the equations to the circle and coaxial helix be

$$\left. \begin{aligned} y &= a \cos \theta \\ z &= a \sin \theta \\ x &= 0 \end{aligned} \right\}$$

and

$$\left. \begin{aligned} y' &= A \cos \theta' \\ z' &= A \sin \theta' \\ x' &= p\theta' \end{aligned} \right\}$$

$$\begin{aligned} \text{Then } M &= \iint \frac{dx dx' + dy dy' + dz dz'}{\sqrt{\{(x-x')^2 + (y-y')^2 + (z-z')^2\}}} \\ &= \int_0^{2\pi} \int_{\theta_1}^{\theta_2} \frac{Aa \cos(\theta - \theta') d\theta d\theta'}{\sqrt{\{A^2 + a^2 - 2Aa \cos(\theta - \theta') + p^2 \theta'^2\}}} \end{aligned}$$

If we change the variables, putting

$$\left. \begin{aligned} \theta - \theta' &= \phi \\ \theta' &= \phi' \end{aligned} \right\}$$

we find

$$M = \int_{-\Theta_2}^{2\pi-\Theta_2} \int_{-\phi}^{\Theta_1} V d\phi d\phi' + \int_{2\pi-\Theta_2}^{\Theta_1} \int_{-\phi}^{2\pi-\phi} V d\phi d\phi' + \int_{-\Theta_1}^{2\pi-\Theta_1} \int_{-\Theta_1}^{2\pi-\phi} V d\phi d\phi'$$

$$\text{where } V = \frac{Aa \cos \phi}{\sqrt{(A^2 + a^2 - 2Aa \cos \phi + p^2 \phi'^2)}} = \frac{Aa \cos \phi}{\sqrt{(\alpha^2 + p^2 \phi'^2)}}$$

$$\text{if } \alpha^2 = A^2 + a^2 - 2Aa \cos \phi.$$

$$\text{Now } \int V d\phi' = \frac{Aa \cos \phi}{p} \log (p\phi' + \sqrt{\alpha^2 + p^2 \phi'^2}) = F(\phi'), \text{ say,}$$

and it may be readily seen by substituting ϕ for $2\pi - \phi$ in the second and fourth integrals that

$$\begin{aligned} - \int_{-\Theta_2}^{2\pi-\Theta_2} F(-\phi) d\phi + \int_{2\pi-\Theta_2}^{\Theta_1} F(2\pi-\phi) d\phi - \int_{2\pi-\Theta_2}^{\Theta_1} F(-\phi) d\phi \\ + \int_{-\Theta_1}^{2\pi-\Theta_1} F(2\pi-\phi) d\phi = 0. \end{aligned}$$

We have, therefore,

$$M = \int_{-\Theta_2}^{2\pi-\Theta_2} F(\Theta_2) d\phi - \int_{-\Theta_1}^{2\pi-\Theta_1} F(\Theta_1) d\phi;$$

hnt

$$\int_{-\Theta_2}^{2\pi-\Theta_2} F(\Theta) d\phi = \int_0^{2\pi} F(\Theta) d\phi, \text{ since } \int_{-\Theta_2}^0 F(\Theta) d\phi = \int_{2\pi-\Theta_2}^{2\pi} F(\Theta) d\phi,$$

therefore

$$\begin{aligned} M &= \int_0^{2\pi} \frac{Aa \cos \phi}{p} \log (p\Theta_2 + \sqrt{\alpha^2 + p^2 \Theta_2^2}) d\phi \\ &\quad - \int_0^{2\pi} \frac{Aa \cos \phi}{p} \log (p\Theta_1 + \sqrt{\alpha^2 + p^2 \Theta_1^2}) d\phi \dots\dots\dots (1). \end{aligned}$$

If $\Theta_1 = 0$ and $\Theta_2 = \Theta$,

$$M_\Theta = \int_0^{2\pi} \frac{Aa \cos \phi}{p} \log \left(p_\alpha^\Theta + \sqrt{1 + \frac{p^2 \Theta^2}{\alpha^2}} \right) d\phi,$$

which is the coefficient of mutual induction of the circle, and a helix beginning in the plane of the circle of axial length, p_α^Θ .

It is clear that $M = M_{\Theta_2} - M_{\Theta_1}$,

and we need therefore only consider the expression

$$M_{\Theta} = \int_0^{2\pi} \frac{Aa \cos \phi}{p} \log \left(\frac{x}{a} + \sqrt{1 + \frac{x^2}{a^2}} \right) d\phi \dots \dots (2),$$

where $x = p\Theta$ = the axial length of the helix, reckoned from the plane of the circle.

We may now proceed in two ways—either by expanding the logarithmic expression in powers of x/a , which leads to a series of limited application since it is convergent only so long as $x < A-a$; or by integration by parts which leads to an expression applicable for all values of x .

§ 3. The first method I developed in the paper above mentioned. We have

$$\begin{aligned} M_{\Theta} &= \frac{Aa}{p} \int_0^{2\pi} \Sigma (-1)^m \frac{1.3.5\dots(2m-1)}{2.4.6\dots 2m} \frac{1}{2m+1} \frac{x^{2m+1}}{a^{2m+1}} \cos \phi d\phi \\ &= \frac{Aa}{p} \Sigma (-1)^m \frac{1.3.5\dots(2m-1)}{2.4.6\dots 2m} \cdot \frac{1}{2m+1} \cdot x^{2m+1} \int_0^{2\pi} \frac{\cos \phi d\phi}{a^{2m+1}} \\ &= 4 \frac{Aa}{p} \Sigma (-1)^{m+1} \frac{1.3.5\dots(2m-1)}{2.4.6\dots 2m} \frac{1}{2m+1} \left(\frac{x}{A+a} \right)^{2m+1} P_m \\ &= \Theta(A+a)c^2 \Sigma (-1)^{m+1} \frac{1.3.5\dots(2m-1)}{2.4.6\dots 2m} \frac{1}{2m+1} g^{2m} P_m \dots (3) \end{aligned}$$

where $c = \frac{2\sqrt{Aa}}{A+a}, \quad g = \frac{x}{A+a},$

$$P_m = \int_0^{\frac{\pi}{2}} \frac{\cos 2\theta d\theta}{(1-c^2 \sin^2 \theta)^{(2m+1)/2}}.$$

Let $Q_m = \int_0^{\frac{\pi}{2}} \frac{d\theta}{(1-c^2 \sin^2 \theta)^{(2m+1)/2}}.$

The following properties of these elliptic integrals are perhaps worthy of notice :—

$$P_m = \left(1 - \frac{2}{c^2} \right) Q_m + \frac{2}{c^2} Q_{m-1} \dots \dots \dots (i),$$

$$Q_m = Q_{m-1} + \frac{c}{2m-1} \dot{Q}_{m-1} \dots \dots \dots (ii),$$

$$P_m = P_{m-1} + \frac{c}{2m-1} \dot{P}_{m-1} \dots\dots\dots (iii),$$

$$(2m+1)c'^2 Q_{m+1} = 2m(1+c'^2)Q_m - (2m-1)Q_{m-1} \dots\dots\dots (iv),$$

$$(2m+1)c'^2 P_{m+1} = 2m(1+c'^2)P_m - \frac{(2m-3)(2m+1)}{2m-1} P_{m-1} \dots\dots\dots (v),$$

$$c'^2 \dot{Q}_m = \dot{Q}_{m-1} + cQ_m \dots\dots\dots (vi),$$

$$cc'^2 \dot{Q}_m = (2m-c'^2)Q_m - (2m-1)Q_{m-1} \dots\dots\dots (vii),$$

$$c'^2 \dot{Q}_m = \left(1+c'^2 + \frac{c^2}{2m-1}\right) \dot{Q}_{m-1} - \dot{Q}_{m-2} \dots\dots\dots (viii),$$

where $c'^2 = 1 - c^2$, and the dotting of a function denotes differentiation with regard to c .

It will be observed that Q_0 and Q_{-1} are respectively the complete elliptic integrals (F and E) of the first and second kinds with regard to modulus c .

§ 4. In equation (3) put

$$K_m = -\frac{1.3.5\dots(2m-1)}{2.4.6\dots 2m} g^{2m} P_m,$$

so that $M_\Theta = \Theta(A+a)c^2 \Sigma (-1)^m K_m$.

Then we can find by a double application of (v) a relation between K_{m+1} , K_m , and K_{m-1} , viz. :—

$$K_{m+1} = \frac{2m(2m+1)}{(2m+2)(2m+3)} d^2 \left\{ K_m - \frac{(2m-1)(2m-3)}{2m \cdot 2m} e^2 K_{m-1} \right\} \dots\dots (4)$$

where $d^2 = \frac{1+c'^2}{c'^2} g^2$ and $e^2 = \frac{1}{1+c'^2} g^2$.

This formula renders the calculation of successive terms of the series sufficiently easy.

§ 5. Hence to find M_Θ , given A , a , and x , we have to calculate the following quantities in order :—

$$c = \frac{2\sqrt{Aa}}{A+a}, \quad \text{and} \quad c' = \sqrt{1-c^2},$$

$$F(c) = \int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{(1-c^2 \sin^2 \theta)}}, \quad \text{and} \quad E(c) = \int_0^{\frac{\pi}{2}} \sqrt{(1-c^2 \sin^2 \theta)} d\theta,$$

$$K_0 = -P_0 = -\left\{ \left(1 - \frac{2}{c^2}\right) F + \frac{2}{c^3} E \right\},$$

$$g^2 = \left(\frac{2}{A+a} \right)^2,$$

$$K_1 = -\frac{1}{2} \cdot \frac{1}{3} g^2 P_1 = -\frac{1}{2} \cdot \frac{1}{3} g^2 \left\{ \left(1 - \frac{2}{c^2} \right) \frac{E}{c'^2} + \frac{2}{c^2} F \right\},$$

$$d^2 = \frac{1+c'^2}{c'^2} g^2, \quad \text{and} \quad e^2 = \frac{1}{1+c'^2} g^2,$$

K_2, K_3, K_4 , &c., by successive applications of (4),

$$\Sigma (-1)^m K_m,$$

and finally $M_\Theta = \Theta (A+a) c^2 \Sigma (-1)^m K_m$.

§ 6. An example may be useful in showing the magnitudes of the various quantities concerned.

If	$2A = 21.02673$ inches,
	$2a = 13.01997$ inches,
	$2x = 5.02480$ inches,
	$\Theta = 201 \pi$,
then	$c = 0.9719540$,
	$c' = 0.2351708$,
	$F = 2.8598352$,
	$E = 1.0655716$,
	$K_0 = 0.9387751$,
	$g^2 = 0.02178146$,
	$K_1 = 0.0561543$,
	$d^2 = 0.4156218$,
	$e^2 = 0.0206400$,
	$K_2 = 0.0076057$,
	$K_3 = 0.0014623$,
	$K_4 = 0.0003387$,
	$K_5 = 0.0000876$,
	$K_6 = 0.0000244$,
	$K_7 = 0.0000071$,
	$\Sigma (-1)^m K_m = 0.8890325$,
	$M_\Theta = 9028.182$ inches $= 22931.166$ cm.

If the circle is in the mean plane of a helix of axial length $2x$, the coefficient of mutual induction will be $2M_\Theta$, or in case of the above dimensions,

$$M = 2M_\Theta = 18056.364 \text{ inches} = 45862.332 \text{ cm.}$$

The value of M_Θ given above was obtained in 1896 by

Mr. Rhodes under the direction of Prof. Ayrton in the Physical Laboratory of the Central Institution, in which the Lorenz apparatus of the McGill University was tested by Prof. Ayrton and myself (Appendix to the Report of the Electrical Standards Committee of the British Association, 1897). The calculation was made by the somewhat laborious method indicated in my paper "On the Determination of the Specific Resistance of Mercury in Absolute Measure."* It was checked by Mr. Mather, and subsequently I calculated M_θ afresh by the method given here.

§ 7. It is important in practice to determine the change in M_θ consequent on small changes in A , a , and x , both for the calculation of the effect on M_θ of small errors of measurement, and because from time to time the disc of any Lorenz apparatus needs to be re-ground in place, and possibly the coil, owing to a breakdown in insulation, may sometimes need to be re-wound.

$$\text{Let } q = \frac{A}{M_\theta} \frac{dM_\theta}{dA}, \quad r = \frac{a}{M_\theta} \frac{dM_\theta}{da}, \quad s = \frac{x}{M_\theta} \frac{dM_\theta}{dx},$$

$$\text{then } \frac{dM_\theta}{M_\theta} = q \frac{dA}{A} + r \frac{da}{a} + s \frac{dx}{x}.$$

It may be shown by direct differentiation using relation (iii) between the P functions and their differential coefficients that

$$\left. \begin{aligned} s &= 2T/W \\ q &= \frac{1-s}{2} + \frac{T+2V}{deW} \\ r &= \frac{1-s}{2} - \frac{T+2V}{deW} \end{aligned} \right\} \dots\dots\dots (5),$$

where

$$W = \Sigma (-1)^m K_m,$$

$$T = \Sigma (-1)^m m K_m,$$

$$V = \Sigma (-1)^m \frac{m}{2m-1} K_m,$$

and d, e have their former significations. W has already been found in calculating M_θ ; and T and V are easily calculable from the known values of K_0, K_1, K_2, K_3 , &c.

Since M_θ is a homogeneous function of the first degree in A, a, x it follows that $q + r + s = 1$, as is obvious in the formulæ above given.

* 'Phil. Trans.,' A (1891), p. 21.

§ 8. For the values of A , a , x given in § 6 we find

$$\begin{aligned} W &= 0.8890325, \\ T &= -0.0443163, \\ 2V &= -0.1036138, \\ \log de &= 2.9667036, \\ s &= -0.0997, \\ q &= -1.2467, \\ r &= +2.3464, \end{aligned}$$

or
$$dM_{\odot}/M_{\odot} = -1.2467dA/A + 2.3464da/a - 0.0997dx/x.$$

Let us suppose that after re-winding the coil mentioned in § 6, and regrinding the disc, the diameters become

$$2A' = 21.02459, \quad 2a' = 13.01499.$$

Then $rdA/a = -0.000899,$

$$qdA/A = +0.000127,$$

and $dM_{\odot}/M_{\odot} = -0.000772$ or $dM_{\odot} = 6.97$ inches = 17.70 cm.

Therefore $M'_{\odot} = 9021.21$ inches = 22913.47 cm.

§ 9. I now pass on to the second and more general method of dealing with the expression—

$$M_{\odot} = \frac{Aa}{p} \int_0^{2\pi} \cos \phi \log \left(\frac{x}{a} + \sqrt{1 + \frac{x^2}{a^2}} \right) d\phi.$$

Integrating by parts we have

$$\begin{aligned} M_{\odot} &= \left[\frac{Aa \sin \phi}{p} \log \left(\frac{x}{a} + \sqrt{1 + \frac{x^2}{a^2}} \right) \right]_0^{2\pi} + \frac{A^2 a^2 x}{p} \int_0^{2\pi} \frac{\sin^2 \phi d\phi}{a^2 \sqrt{(a^2 + x^2)}} \\ &= A^2 a^2 \Theta \int_0^{2\pi} \frac{\sin^2 \phi d\phi}{a^2 \sqrt{(a^2 + x^2)}} \\ &= \frac{16 A^2 a^2 \Theta}{(A+a)^2 \sqrt{\{(A+a)^2 + x^2\}}} \int_0^{\frac{\pi}{2}} \frac{\sin^2 \psi \cos^2 \psi d\psi}{(1 - c^2 \sin^2 \psi) \sqrt{(1 - k^2 \sin^2 \psi)}}, \end{aligned}$$

where $c^2 = \frac{4Aa}{(A+a)^2}$, and $k^2 = \frac{4Aa}{(A+a)^2 + x^2}.$

Hence by simple changes we have

$$M_{\odot} = \Theta (A+a) ck \left\{ \frac{F-E}{k^2} + \frac{c'^2}{c^2} (F-11) \right\} \dots \dots (6),$$

where F and E are complete elliptic integrals of the first and second kinds to modulus k ,

$$\text{and} \quad \Pi = \int_0^{\frac{\pi}{2}} \frac{d\psi}{(1-c^2 \sin^2 \psi) \sqrt{(1-k^2 \sin^2 \psi)}}.$$

This expression for M_θ is applicable for all values of x from 0 to ∞ . The elliptic integral of the third kind Π is expressible by Legendre's formula in terms of complete and incomplete integrals of the first and second kind. Thus, if we put

$$-c^2 = -1 + k'^2 \sin^2 \beta,$$

$$\text{or} \quad \sin \beta = c'/k',$$

we have (v. Cayley, "Elliptic Integrals," § 183)

$$\begin{aligned} & \frac{k'^2 \sin \beta \cos \beta}{c} (F - \Pi) \\ &= -\frac{\pi}{2} - F(k) F(k', \beta) + E(k) F(k', \beta) + F(k) E(k', \beta). \end{aligned}$$

These elliptic integrals, complete and incomplete, may be conveniently calculated by successive quadric transformations, as shown in Cayley, Chap. XIII.

§ 10. Taking for the diameters of helix and circle, the values given in § 8, viz.,

$$2A' = 21.02459, \quad 2a' = 18.01499,$$

$$\text{and as before} \quad 2x = 5.02480,$$

Professor Ayrton and I made the calculation of M_θ by equation (6) with the following results:—

$$\begin{aligned} c &= 0.9719222, \\ k &= 0.9615024, \\ k' &= 0.2747959, \\ L \sin \beta &= 9.9826156, \\ F(k) &= 2.7109750, \\ E(k) &= 1.0840174, \\ F(k', \beta) &= 1.0393881, \\ E(k', \beta) &= 1.0168643, \\ \frac{k'^2 \sin \beta \cos \beta}{c} (F - \Pi) &= -0.5051433, \\ \frac{c'^2}{c^2} (F - \Pi) &= -0.8616240, \\ \frac{F - E}{k^2} &= 1.7598494, \\ M_\theta &= 22913.59. \end{aligned}$$

This result afforded a sufficiently satisfactory proof of the accuracy of the calculations, seeing that the result in § 8 was obtained by the application of equations (3) and (5), and that it approximated so closely to the result just given. The approximation would be still closer if we had taken account of the neglected terms of the series, K_s and K_3 .

§ 11. To find corresponding general expressions for the rates of variation of M_Θ with A , a , and x it is simplest to revert to the expression

$$M_\Theta = \frac{Aa}{p} \int_0^{2\pi} \cos \phi \log \left(\frac{x}{a} + \sqrt{1 + \frac{x^2}{a^2}} \right) d\phi.$$

We have
$$\frac{dM_\Theta}{dx} = -\frac{M_\Theta}{x} + \frac{Aa\Theta}{x} \int_0^{2\pi} \frac{\cos \phi d\phi}{\sqrt{(a^2 + x^2)}}$$

$$(\text{putting } p = x/\Theta),$$

$$= -\frac{M_\Theta}{x} + \frac{\Theta(A+a)ck}{x} P_0(k),$$

$$\frac{dM_\Theta}{dA} = \frac{M_\Theta}{A} - Aa\Theta \int_0^{2\pi} \cos \phi \frac{A - a \cos \phi}{a^2 \sqrt{(a^2 + x^2)}} d\phi,$$

$$= -Aa\Theta \int_0^{2\pi} \frac{A \cos \phi - a}{a^2 \sqrt{(a^2 + x^2)}} d\phi,$$

$$= \Theta ck \left\{ F + \frac{A-a}{2a} (F - \Pi) \right\},$$

where F and Π have the same significations as in equation (6).

$$\text{Similarly } \frac{dM_\Theta}{da} = \Theta ck \left\{ F - \frac{A-a}{2A} (F - \Pi) \right\},$$

$$\text{Hence } \left. \begin{aligned} q &= \frac{\Theta ck}{M_\Theta} A \left\{ F + \frac{A-a}{2a} (F - \Pi) \right\} \\ r &= \frac{\Theta ck}{M_\Theta} a \left\{ F - \frac{A-a}{2A} (F - \Pi) \right\} \\ s &= -1 - \frac{\Theta ck}{M_\Theta} (A+a) \left\{ \left(1 - \frac{2}{k^2} \right) F + \frac{2}{k^2} E \right\} \end{aligned} \right\} \dots (7).$$

It is readily deducible from equations (7) that

$$q + r + s = 1.$$

When M_0 has been calculated by (6) it is a simple matter to calculate q, r, s by the above equations.

Let $\frac{M_0}{\Theta(A+a)ck} = Z$. Then equations (7) may be expressed thus:—

$$\left. \begin{aligned} q &= \frac{A}{2aZ} (F - c'\Pi) \\ r &= \frac{a}{2AZ} (F + c'\Pi) \\ s &= -1 - P_0(k)/Z \end{aligned} \right\} \dots\dots\dots (8).$$

For the values of $2A', 2a', 2x$ taken in § 10 we have

$$\begin{aligned} \Pi &= 17\cdot411370, \\ \Pi c' &= 4\cdot096932, \\ F - \Pi c' &= -1\cdot385957, \\ F + \Pi c' &= 6\cdot807907, \\ Z &= 0\cdot8982254, \\ P_0(k) &= -0\cdot8087238, \\ q &= -1\cdot24629, \\ r &= +2\cdot34593, \\ s &= -0\cdot09964. \end{aligned}$$

On the Potential Energy of a Circular Current and a Uniform Coaxial Circular Cylindrical Current Sheet.

§ 12. The current lines in the sheet are circles in planes at right angles to the axis.

Let the circle have its centre at the origin, and let its equations be

$$\left. \begin{aligned} y &= a \cos \theta \\ z &= a \sin \theta \end{aligned} \right\}$$

and let the equations to the coaxial cylindrical sheet be

$$\left. \begin{aligned} y' &= A \cos \theta' \\ z' &= A \sin \theta' \end{aligned} \right\}$$

the plane ends of the cylinder being determined by the equations $x = x_1, x = x_2$.

Let the current in the circle be γ_0 , and the current per unit length of the cylindrical sheet γ ; and let the potential energy of the circular current and the current sheet be M' .

$$\begin{aligned} \text{Then } M' &= \gamma_c \gamma \int_{x_1}^{x_2} dx \int_0^{2\pi} d\theta \int_0^{2\pi} d\theta' \frac{Aa \cos(\theta - \theta')}{\sqrt{A^2 + a^2 - 2Aa \cos(\theta - \theta') + x^2}} \\ &= 2\pi \gamma_c \gamma \int_0^{2\pi} d\phi \int_{x_1}^{x_2} dx \frac{Aa \cos \phi}{\sqrt{(a^2 + x^2)}} \end{aligned}$$

$$\begin{aligned} \text{or } M' &= 2\pi \gamma_c \gamma Aa \left[\int_0^{2\pi} \cos \phi \log \left(\frac{x_2}{a} + \sqrt{1 + \frac{x_2^2}{a^2}} \right) d\phi \right. \\ &\quad \left. - \int_0^{2\pi} \cos \phi \log \left(\frac{x_1}{a} + \sqrt{1 + \frac{x_1^2}{a^2}} \right) d\phi \right]. \end{aligned}$$

For a circle and coaxial helix we find the potential energy by multiplying their coefficient of mutual induction by the product of γ_c , γ_h , the currents in the circle and helix respectively. Taking the expression for the coefficient of mutual induction

$$M = M_{\theta_2} - M_{\theta_1},$$

M_{θ_2} and M_{θ_1} being expressed by equation (2), and noting that $\gamma_h = 2\pi p \gamma$ where γ is the current per unit length of the helix measured parallel to the axis, we see that the potential energy of the circle and coaxial helix is identically the same as the potential energy of the circle and a uniform coaxial circular cylindrical current sheet of the same radial and axial dimensions as the helix, if the currents per unit length in helix and sheet be the same.*

On the Potential Energy of a Helical Current and a Uniform Coaxial Circular Cylindrical Current Sheet.

§ 13. If we attempt to integrate the general expression for the coefficient of mutual induction in the case of two coaxial helices we are brought face to face with functions which indeed deserve the attention of mathematicians, but do not at present lend themselves to calculation.

For many practical purposes, however, it will be equally useful to calculate the potential energy of a helix and a coaxial uniform circular cylindrical current sheet, or of two coaxial uniform circular cylindrical current sheets; and this potential energy is expressible in terms of elliptic integrals.

Let the equations to a helical current be as before,

$$\left. \begin{aligned} y' &= A \cos \theta' \\ z' &= A \sin \theta' \\ x' &= p\theta' \end{aligned} \right\},$$

* I.e., if the current across a generating line of the sheet equal to the pitch of the helix is equal to the helical current.—J. V. J., April 21, 1898.

the limits of x' being x'_1 and x'_2 , so that the axial length of the helix is $x'_2 - x'_1$.

Let the equations to a coaxial circular cylindrical current sheet be

$$y = a \cos \theta, \quad z = a \sin \theta,$$

its ends being at distances x_1, x_2 from the origin.

Let γ_h be the current in the helix, and let γ be the current per unit length of the uniform current sheet. Then if M' is the potential energy of the helical current and the current sheet we have

$$\begin{aligned} M' &= \int_{x_1}^{x_2} \gamma_h \gamma dx \int_0^{2\pi} \frac{Aa \cos \phi}{p} \left[\log \left(\frac{x'_2 - x}{a} + \sqrt{1 + \frac{(x'_2 - x)^2}{a^2}} \right) \right. \\ &\quad \left. - \log \left(\frac{x'_1 - x}{a} + \sqrt{1 + \frac{(x'_1 - x)^2}{a^2}} \right) \right] d\phi \\ &= \frac{Aa}{p} \gamma_h \gamma \int_0^{2\pi} \cos \phi d\phi [f(x'_2 - x_1) - f(x'_2 - x_2) + f(x'_1 - x_2) - f(x'_1 - x_1)] \end{aligned}$$

where $f(z) \equiv z \log \left(\frac{z}{a} + \sqrt{1 + \frac{z^2}{a^2}} \right) - \sqrt{a^2 + z^2}$.

The integral $\int_0^{2\pi} \cos \phi f(z) d\phi$ is easily reducible to elliptic integrals of standard form.

It is to be noticed that $f(z) = f(-z)$.

If the axial length of the helix $= 2l$, and the axial length of the cylindrical sheet $= 2m$, and if \bar{x} equals the distance between their mean planes, we have

$$\begin{aligned} M' &= \frac{Aa}{p} \gamma_h \gamma \int_0^{2\pi} \cos \phi d\phi [f(\bar{x} + l + m) - f(\bar{x} + l - m) \\ &\quad + f(\bar{x} - l - m) - f(\bar{x} - l + m)] \dots \quad (9). \end{aligned}$$

Similarly for two coaxial cylindrical current sheets we have

$$\begin{aligned} M' &= 2\pi Aa \gamma \gamma' \int_0^{2\pi} \cos \phi d\phi [f(\bar{x} + l + m) - f(\bar{x} + l - m) \\ &\quad + f(\bar{x} - l - m) - f(\bar{x} - l + m)] \dots \quad (10), \end{aligned}$$

where γ, γ' are the currents per unit length in the sheets.

§ 14. To find the force between a helical current and a coaxial circular cylindrical uniform current sheet we have to differentiate the potential energy as expressed in equation (9) with regard to x .

We have, if X is the force,

$$X = \frac{dM'}{dx} = \gamma_1 \gamma \frac{Aa}{p} \int_0^{2\pi} \cos \phi \, d\phi [F(\bar{x}+l+m) - F(\bar{x}+l-m) \\ + F(\bar{x}-l-m) - F(\bar{x}-l+m)],$$

where $F(z) = f'(z) = \log \left(\frac{z}{a} + \sqrt{1 + \frac{z^2}{a^2}} \right),$

or by equation (2)

$$X = \gamma_1 \gamma (M_2 - M_1) \dots\dots\dots (11),$$

where M_2 = coefficient of mutual induction of the helix and one of the circular ends of the sheet,

and M_1 = coefficient of mutual induction of the helix and the other circular end of the sheet.

M_2 and M_1 may be calculated as described in the previous articles of this paper.

§ 15. Equation (11) is clearly a particular case of a more general theorem.

Take any cylindrical sheet developed by the rectilinear translation of a given closed curve, and let the sheet be the seat of a uniformly distributed current, the current lines being successive positions of the given curve as by its translation it develops the sheet. Let the current per unit length of the sheet be γ . Further, let M_1 be the coefficient of mutual induction of the given curve in its first position and any second fixed curve, and M_2 the coefficient of mutual induction of the given curve in its last position and the second curve; and let γ_2 be the current in the second curve.

Then the force between the current sheet and the second curve resolved parallel to the direction of translation of the given curve as it develops the sheet is given by the formula—

$$F = \gamma_2 \gamma (M_2 - M_1).$$

For let the direction of translation be taken as the axis of x ; and let M_x be the coefficient of mutual induction of the given curve in any intermediate position defined by the co-ordinate x , i.e., of an element of the current sheet, and the second curve. Then the force resolved parallel to x between the element of the current sheet and the second curve

$$= \gamma_2 \cdot \gamma dx \cdot dM_x/dx,$$

and the total force so resolved between the current sheet and the second curve

$$= \int_{x_1}^{x_2} \gamma_2 \cdot \gamma dx \cdot dM_z/dx = \gamma_2 \gamma (M_2 - M_1).*$$

As special cases, we have equation (11) reducing the calculation of the force between a circular cylindrical uniform current sheet and a coaxial helical current to the calculation of the coefficients of mutual induction of the helix and the circular ends of the sheet; and the simpler case of the force between a circular cylindrical uniform current sheet and a circular current, which is obtained from the calculations of the coefficients of mutual induction of the circle and the circular ends of the sheet.

I hope that equation (11) may be of service in the accurate calculation of the constants of current weighing apparatus. My attention was drawn to the matter from this point of view in consequence of the Report of the Electrical Standards Committee of the British Association made at Toronto, in which mention is made of the importance of re-determining the ampere.

March 31, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Preliminary communications upon the results of the recent Solar Eclipse were made by the following members of the expeditions :—

The Astronomer Royal, Sir J. Norman Lockyer, K.C.B., Professor H. H. Turner, Dr. R. Copeland, and Captain E. H. Hills, R.E., and Mr. H. F. Newall.

The Society adjourned over the Easter Recess to Thursday, April 28.

* Professor A. Gray has pointed out to me that this result may be deduced from the consideration that the removal of an element from one end of the sheet to the other is equivalent to a small motion of the sheet parallel to its generating lines.
—J. V. J., April 21, 1898.

April 28, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The following Papers were read:—

- I. "On the Meteorological Observatories of the Azores." By H.S.H. PRINCE ALBERT I OF MONACO. Communicated by J. Y. BUCHANAN, F.R.S.
- II. "A Compensated Interference Dilatometer." By A. E. TUTTON. Communicated by Professor CLIFTON, F.R.S.
- III. "A Calorimeter for the Human Body." By WILLIAM MARCET, M.D., F.R.S.
- IV. "An Experimental Enquiry into the Heat given out by the Human Body." By WILLIAM MARCET, M.D., F.R.S.

"On the Meteorological Observatories of the Azores." By H.S.H. THE PRINCE ALBERT I OF MONACO. Communicated by Mr. J. Y. BUCHANAN, F.R.S. Received April 19, —Read April 28, 1898.

(Translation.)

In 1892 I brought before the British Association, met in Edinburgh, a project which my scientific cruises in the North Atlantic had suggested and preliminary experiments have matured. The importance of meteorological observations is now universally recognised, and a continually increasing number of centres of observation are being created in order to assist the progress of this science.

I proposed to establish on the Azores an advanced post, whose mission should be: 1st, to observe the birth of certain atmospheric disturbances, which appear to be formed in this region of the Atlantic; 2nd, to correct the path of certain others which appear to threaten the coasts of Europe and which are announced from America at too great a distance of time and space for there to be an assurance that more or less considerable modifications may not take place which will affect their strength, their direction, and the date of their arrival on the European coasts.

We should then have, on a point situated almost in the middle of

the Atlantic, an observatory, the work of which would also include the study of seismic phenomena, and this with great advantage, because, in certain circumstances, the earthquakes felt in Europe have, in the first instance, affected the volcanic *massif* of the Azores.

It is hardly necessary to say that those interested in the study of terrestrial magnetism will view with pleasure the opening of an observatory which is to render useful services in this department of science. Many countries will profit directly by the observations made on the Azores, because they interest all branches of the nautical profession as well as the populations of all the western coasts of Europe. The navigator approaching the end of a long voyage in a sailing vessel will here find the means of regulating his chronometers at the time when it is most wanted, namely, before making the continental land.

Since the first mention of my project in 1892, an event, which I awaited with much impatience, has arrived to help it, namely, the telegraphic connection of the Azores with Europe. Soon afterwards the Portuguese Government gave effect to my views by creating on the Island of St. Miguel, and under the direction of Captain Chaves, a regular meteorological station, although fitted with the most modest means.

Finally, last year Captain Chaves was commissioned to establish on the Island of Flores, the most westerly of the Azores, a second meteorological station, the observations at which will be most useful for supplementing those of San Miguel. Unfortunately, its means are even more modest than those of the preceding one, and the telegraph cable has not yet reached it.

In spite of the insufficient means of observation actually existing in the Azores, the results which they already furnish towards establishing the paths of the depressions which cross the Atlantic, enable a presentiment to be formed of the part which they will play when their outfit is more complete.

In order that science may the sooner profit by the advantages promised by the observatory of the Azores; and in order to guarantee it against all the dangers which might cause an interruption of its functions, I propose to give it a constitution founded on the principle of an international guarantee, which would be secured by the pecuniary contributions of the countries interested. It is principally to expose this idea that I have come to-day to speak of these observatories to the Royal Society.

Portugal, accepting the principle of the international *régime*, has commissioned Captain Chaves to invite the maritime nations interested to give their adhesion to my project and to associate themselves in the organisation of the above-mentioned meteorological service.

It may therefore be hoped that in the near future an understanding will be arrived at on the importance of the development to be given to the observatory of the Azores, and I ask the Royal Society, whose influence is so great in the domain of science, to support, by its concurrence, the accession of England to the ideas which I uphold for the common interest.

“A Compensated Interference Dilatometer.” By A. E. TUTTON, Assoc. R.C.S. Communicated by Captain ABNEY, C.B., F.R.S. Received March 8,—Read April 28, 1898.

(Abstract.)

The author describes a form of Fizeau interference dilatometer which he considers combines the best features of the apparatus described by Benoit, and belonging to the Bureau International des Poids et Mesures, in Paris,* and that described by Pulfrich† constructed according to the modifications introduced into the method by Abbe. Moreover, besides other improvements, a new principle, that of compensation for the expansion of the screws of the Fizeau tripod which supports the object, is introduced, which enhances the sensitiveness of the method so highly as to render it applicable to the determination of the expansion of crystals in general, including those of chemical preparations. Hitherto the application of the Fizeau method has been confined to such crystals as could be obtained large enough to furnish a homogeneous block at least a centimetre thick. A block only 5 mm. thick is ample for use with the author's compensated dilatometer. The principle of the compensation depends upon the fact that aluminium expands 2·6 times as much as platinum-iridium for the same increment of temperature. The author therefore employs, like Fizeau and Benoit, a tripod of platinum-iridium, and places upon its transverse table, through which pass the three screws, a disc of aluminium whose thickness is $1/2\cdot6$ ths of the length of the screws. The space between the lower surface of the glass plate which is laid upon the upper ends of the screws to assist in producing the interference, and the upper surface of the aluminium, then remains constant for all temperatures under observation, and if a crystal is laid upon the aluminium compensator the whole amount of its expansion by rise of temperature is available for measurement by the interference method. Hence the method is no longer a merely relative one, affording the difference of expansion between the tripod and the substance investigated, but affords directly absolute measurements of the expansion.

* ‘Trav. et Mémoires,’ 1881, p. 1.

† ‘Zeits. für Instrumen.,’ 1893, p. 365.

The instrument consists of two independently mounted portions, which the author terms respectively the expansion apparatus and the illuminating and observing apparatus. For the purposes of preliminary adjustment the two portions are arranged at close quarters, while during the observations they stand at opposite ends of the slate table, 6 feet in length, upon which they are mounted, the expansion apparatus resting on a separate and movable cloth-lined slab for the purpose of effecting the transfer from one position to the other. Hence the optical measuring apparatus is far removed during observations from the heated atmosphere of the chamber containing the tripod.

The illuminating and observing apparatus consists of a telescope arranged with a side tube for auto-collimation. It is mounted on a stout pedestal provided with three legs and levelling screws, and its height can be varied by the vertical rack and pinion movement of a stout inner column. At the common focus of the lens of the side tube and of the telescope objective a small totally-reflecting prism is placed, half closing the aperture of a diaphragm placed on the objective side of the prism in the main optical tube; the prism is so arranged that the light from the illuminating lens is reflected through the diaphragm to the objective. It passes thence, as parallel rays, across the intervening space to the expansion apparatus, at the summit of which it meets with one of two interchangeable deflecting arrangements which direct the rays vertically down the tube of the expansion apparatus into the interference chamber containing the tripod. One of the two is a large totally-reflecting prism; this is used when white light or a sodium flame is the source of light placed before the illuminating lens, the former for adjusting purposes and the latter for generating the bands. The other is a train of two refracting prisms whose total minimum deviation averages 90° ; this is used when a hydrogen and mercury Geissler tube is the source of light; the dispersion being then adequate to effectively separate the red C from the greenish-blue F radiations, or both from the mercury green radiation, when it is desired to generate bands in C or F hydrogen light, or that corresponding to the green mercury line as recommended by Pulfrich.

The expansion apparatus is suspended from an arm carried by a pedestal provided with rack and pinion vertical adjustment as in the case of the observing apparatus. Below the deflecting prism or prisms the short metal tube passes into a longer one of porcelain, which at its lower end is fitted into a further short metal tube carrying below the interference chamber within which the tripod is placed. On passing down the tube from the deflecting apparatus the rays pass first through a slightly tilted, thick glass disc held in a diaphragm and forming the roof of the interference chamber. They

then pass through the large cover-disc laid on the tripod screws, the under surface of which is one of the two surfaces the reflections from which are to be made to interfere; the other of the two relevant surfaces is the upper surface of the crystal supported on the compensator, or in the case of a badly reflecting crystal, of a small disc of black glass, polished above and ground below, which the author lays upon it. The large cover-disc and the glass roof-disc are slightly wedge-shaped to the extent of 35', and are arranged complementarily so as to counteract the dispersion produced by each other; by using a wedge-shaped cover-disc the undesired reflection from the upper surface can be deflected out of the field of the telescope, and the tilt of the roof-wedge is given for a similar reason. After reflection from the two surfaces relevant to the interference, the rays re-traverse their path, but instead of doing so absolutely are made to pass through the clear half of the telescope diaphragm to the observing lens arrangement. An iris diaphragm is placed against the main diaphragm to assist in further excluding undesirable radiations, and the illuminated surface of the small reflecting prism can be more or less curtailed by suitable rectangular signal-stops.

There are two observing lens arrangements: one a simple eyepiece and the other a micrometer combination of three lenses. The first enables the observer to properly adjust the images of the signal stop in white light reflected from the two relevant surfaces of the interference apparatus, so as to cover each other to the extent required to produce interference bands of requisite width. The micrometer combination converts the observing apparatus into a microscope wherewith to view the bands. The spider-lines of the micrometer are simultaneously visible. The reference point of the interference apparatus is the centre of a minute silver ring on the under surface of the cover-wedge, and the two vertical spider-lines can be adjusted by a special drum to such a separation as enables the inner circle of the ring to be brought symmetrical to them, showing equal suitable arcs outside each; this separation is also convenient for the width of band generally employed, which corresponds to 100 drum divisions of the other drum which moves both spider-lines simultaneously.

The interference chamber is provided with an adjusting table of non-conducting material, and is quite open to the heated air of the bath, being provided with large windows, which are also very useful for the adjustment of the tripod. The heating bath is a double air bath of copper suitably screened in every direction by asbestos millboard. It is provided with a thermostat in the outer bath and two thermometers in the inner bath. The expansion apparatus is immersed in the latter up to a third of the porcelain tube. The *actual temperature* of the tripod is ascertained by a third thermo-

meter, bent so that the bulb lies within the chamber in contact with the tripod itself; this has been found to be a point of the first importance. With the aid of the thermostat and a graduated gas tap a constant temperature can readily be attained.

The author determines the position of the bands at about 10°C. , again near 70° , and once more about 120° , in order that not only the mean coefficient, but the absolute coefficient at any temperature and the increment per degree may be ascertained. The transit of each band is followed and recorded permanently by means of a specially constructed tape-puncturing recorder. This method is found more satisfactory than relying exclusively on the Abbe method of mere observation of the initial and final positions of the bands for light of two wave-lengths.

The results of numerous determinations of the expansion of the platinum-iridium of the tripod are given, carried out with the surface of the tripod table and the cover-wedge separated at the long interval of 12 mm., by the aid of green mercury light. The mean value is very similar to that of Benoit, and is

$$\alpha = 10^{-6}(8600 + 4.56t).$$

The result of several determinations in red hydrogen light of the expansion of the pure aluminium used for the series of compensators, carried out by the Fizeau relative method with a block 12 mm. thick, is

$$\alpha = 10^{-6}(2204 + 2.12t).$$

Similar determinations for the black glass of the crystal-covering plates afford the value:

$$\alpha = 10^{-6}(7257 + 10.4t).$$

In a subsequent memoir the author intends to present the results of determinations of the expansion of the sulphates and selenates of potassium, rubidium, and caesium.

May 5, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

In pursuance of the Statutes, the names of the Candidates recommended by the Council for election into the Society were read as follows:—

Baker, Henry Frederick, M.A.	Preston, Professor Thomas, M.A.
Brown, Professor Ernest William.	Reid, Professor Edward Waymouth, M.B.
Buchan, Dr. Alexander, M.A.	Scott, Alexander, M.A.
Harmer, Sidney Frederic, M.A.	Seward, Albert Charles, M.A.
Lister, Arthur, F.L.S.	Shenstone, William Ashwell, F.I.C.
McMahon, Lieutenant - General Charles Alexander.	Taylor, Henry Martyn.
Osler, Professor William, M.D.	Wimshurst, James.
Parsons, Hon. Charles A., M.A.	

The following Papers were read:—

- I. "Observations on the Action of Anæsthetics on Vegetable and Animal Protoplasm." By Dr. A. D. WALLER, F.R.S., and Professor J. B. FARMER.
- II. "On certain Structures formed in the drying of a Fluid with Particles in Suspension." By Miss C. A. RAISIN. Communicated by Professor BONNEY, F.R.S.
- III. "On Photographic Evidence of the Objective Reality of Combination Tones." By R. W. FORSYTH and R. J. SOWTER. Communicated by Professor RÜCKER, Sec. R.S.
- IV. "The Relations between the Hybrid and Parent Forms of Echinoid Larvæ." By H. M. VERNON. Communicated by Professor LANKESTER, F.R.S.

“Observations on the Action of Anæsthetics on Vegetable and Animal Protoplasm.” By J. B. FARMER, M.A., and A. D. WALLER, M.D., F.R.S. Received March 9,—Read May 5, 1898.

The object in view was to observe simultaneously and comparatively the effects of certain anæsthetics (carbon dioxide, ether, and chloroform) upon vegetable and upon animal protoplasm.

Two gas chambers in series, through which anæsthetic and other vapours can be passed, contain: the first, a leaf of *Elodea Canadensis* under the microscope ($\times 300$); the second, a sciatic nerve of *Rana temporaria* connected with an inductorium and galvanometer (or upon occasion a galvanograph).*

The actual movements of chlorophyll bodies in a cell of the leaf were observed and measured by one of us, while the other observer took readings of the galvanometric deflections in response to excitation of the nerve. To establish comparison between the two classes of effects, we took as measures:—the number of chlorophyll bodies that crossed a cobweb in the eye-piece during each successive minute, and the magnitude of galvanometric deflections at intervals of one minute, before, during, and after the action of the vapour. The number of bodies passing per minute gives measure of the rate of movement in the vegetable protoplasm, while the magnitude of successive galvanometric deflections gives measure of the mobility of the animal protoplasm.

Our results will be most briefly presented by the records of some representative observations.

Experiment I.

	<i>Chara.</i>	<i>Nerve.</i>
Chloroform vapour, 5 per cent. for 2 minutes.	Permanent abolition of movement.	Temporary abolition of mobility.

Experiment II.

Weak ether vapour for 10 minutes.	No marked effect.	No marked effect.
Stronger ether vapour for 4 minutes.	Permanent abolition.	Temporary diminution.

* As described in ‘Phil. Trans.,’ B, vol. 188 (1897), p. 4.

Experiment III.

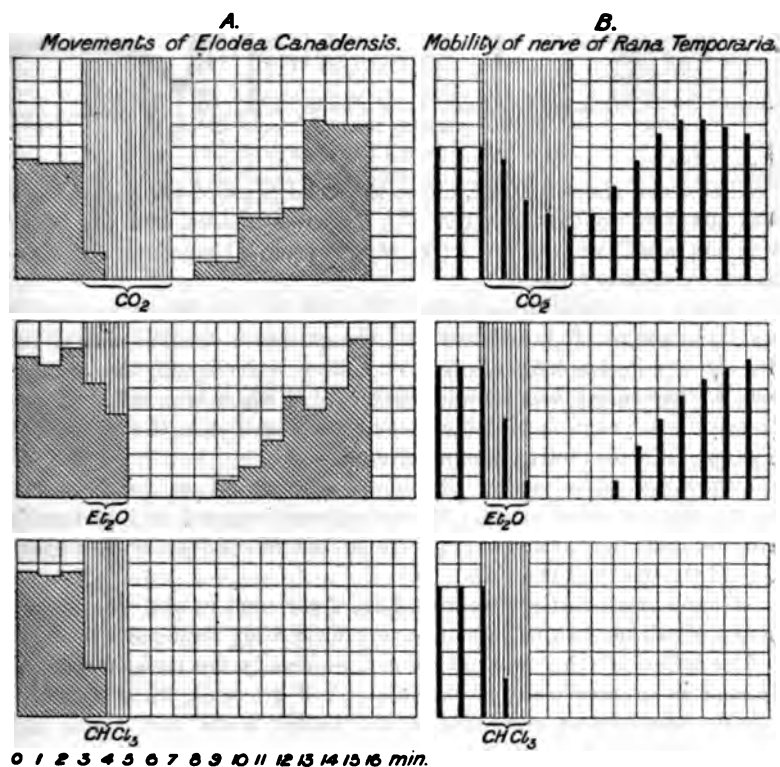
	<i>Elodea.</i>	<i>Nerve.</i>
Nitrous oxide for 5 minutes.	Diminution of movement.	Diminution of responses.
Hydrogen for 5 minutes.	Diminution.	Diminution.
Carbon dioxide for 3 minutes.	Arrested, followed by more rapid movement.	Diminution from 15 to 5, followed by augmentation to 35.
Carbon dioxide for 4 minutes.	Ditto.	Diminution, followed by augmentation.
Carbon dioxide for 2 minutes.	Ditto.	Ditto.

Experiment IV.

<i>Elodea.</i>					
	Rate of movement, indicated by number of chlorophyll granules passing per minute.		Rate of movement, indicated by number of chlorophyll granules passing per minute.		
	Time.		Time.		
	1	18	19	12	
	2	15	20	20	
CO ₂	{ 3	4 }	21	26	
	{ 4	0 }	22	26	
	5	0	23	26	
	6	0	24	26	
	7	6	CO ₂	{ 25	6 }
	8	6		{ 26	0 }
	9	6		{ 27	0 }
	10	16		{ 28	0 }
	11	26	29	0	
	12	17	30	4	
	13	15	31	4	
CO ₂	{ 14	19 }	32	14	
	{ 15	0 }	33	16	
	16	13	34	36	
	17	1	35	35	
	18	0			

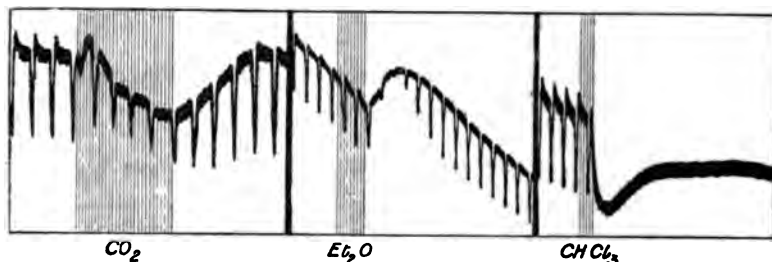
Experiments V, VI, and VII.

Action of Carbon Dioxide, of Ether, and of Chloroform upon Vegetable and Animal Protoplasm.



The results obtained from a study of *Chara* and *Elodea* were quite consistent, but owing to the greater ease in making a quantitative determination, the latter plant was used for the more exact comparative experiments.

The action of carbon dioxide was to produce an initial slight acceleration, followed speedily by a complete cessation of movement. After disconnecting the CO_2 apparatus and aspirating air through the chamber the protoplasm, after the lapse of two or three minutes, began to show signs of recovery. Fitful movements of the granules first occurred, and then they soon resumed their processional motion around the cell. At first very slow, the movement rapidly became accelerated and considerably exceeded the normal rate. This acceleration was not of long duration, and was followed by a slowing down to the ordinary speed.



The nerve showed itself, under the conditions of the experiment, less sensitive to the action of CO_2 than was *Elodea*, and the latter was less sensitive than an active *Myxomycete* plasmodium (of *Badhamia*) similarly treated.

Ether vapour in air passed over the plant for two minutes caused a speedy arrest of all movement, and the quiescent condition persisted for some minutes longer. Recovery then ensued and the normal rate of movement was slowly regained. With dilute ether vapour (below 10 per cent. in air) insufficient to anæsthetise the nerve, the protoplasmic circulation was unaffected.

Chloroform.—The action of chloroform proved to be far more deadly than that of ether. Movement was arrested in less than a minute, and two minutes' exposure to the full action of its vapour caused the death of the cell.

When a more diluted vapour (about 2 per cent. in air) was passed over the cell for two minutes recovery ultimately occurred.

The action of ether and chloroform, especially the latter, was very marked in causing many of the chlorophyll granules, which had previously been almost restricted to the lateral walls, and hence had presented their edges to the incident light, to become dispersed over the surface of the cell, where they were fully exposed, over their largest area to the light. The action of carbon dioxide as observed in these experiments was not nearly so pronounced. This phenomenon is such as might have been anticipated as a result of the paralysis, temporary or permanent, of the protoplasm.

“On certain Structures formed in the drying of a Fluid with Particles in Suspension.” By CATHERINE A. RAISIN, B.Sc.
Communicated by Professor T. G. BONNEY, D.Sc., LL.D.,
F.R.S. Received March 16,—Read May 5, 1898.

(PLATE 2.)

- PART I.—1. Origin and Method of Experiments.
2. Classification of Forms produced.
3. Conditions and Causes of Formation.
4. Possible Applications in Nature.
PART II.—5. Results affected by Crystallisation.

PART I.

1. *Origin and Method of Experiments.*

I have frequently had to mount in water, for examination with the microscope, the powder of various rocks. Certain of the slides, when accidentally dried, exhibited rather interesting forms. These I showed to Professor Bonney, who encouraged me to try for further results, so the experiments were continued with various powdered substances—many pigments (vermilion, indigo, sepia, &c.), chalk and other more or less friable rocks. At first, ordinary microscope slides were used, but afterwards, larger pieces of glass, in each case, generally with a cover-glass placed over the mud. The results seem to be worth describing, as affording familiar, almost homely illustrations which may throw some light on the origin of certain minor structures in rocks.

2. *Classification of Forms.*

The chief effects produced may be shortly described. One form shown by the dried powder is that of a winding network (Plate 2, fig. 1), which is formed of bent stems, fairly uniform in thickness, giving rise to short branches in different directions, but generally at high angles. The whole makes a kind of maze in which the broader winding spaces often average from $\frac{1}{12}$ inch to $\frac{1}{8}$ inch in width. This form arises from desiccation of a fairly dense* mass. Where it is densest, a coarser maze is developed and the coarser part is earlier formed† (fig. 1 towards *a* and *b*).

* I use this term for a mass where the water is distinctly “muddy,” i.e., where the proportion of the suspended particles to the water is relatively high.

† Sometimes the denser mud has been carried forward apparently by the squeezing along of more suspended material (fig. 1). Sometimes a greater amount of the solid substance has been left behind, while a less dense mass was pressed forward. In slides left to dry in a tilted position, gravitation carried the greater amount of particles towards the lower end.

The meshes in the network may be occupied by a finer deposit arranged in successive curves. Within a coarser maze, the curves are closer, and the finer material forms a more conspicuous pattern.

In other examples, the thick streaks become less bent (*cf.* fig. 4; also fig. 2), and finally straight, sometimes with a terminal knob, sometimes forked or branched, generally at a rather acute angle. From these axes, finer pattern-marks seem to diverge on either side. Certain materials usually formed straight axes*—these were also developed in other substances under certain conditions, *e.g.*, when the mud was thin, or when its boundary was within the edge of the cover glass, or when a corner of the cover was upraised to allow quicker escape of the moisture. Thus the straight axes seem to be the result of a more ready retreat of the rim generally due to a freer evaporation. While, if the cover was sealed down with canada balsam, or if denser mud had spread beyond the edge and caked, so as to check evaporation,† a maze was produced.

A fine pattern may be formed either in connection with the thicker aggregates, or where these are wanting. It generally arises from a sorting of the material—the finer feathers and tufts are built of small grains, the thick coarse axes of larger. The fine particles may be spread almost continuously (*e.g.*, ivory black, light red), the deposit then becoming more concentrated towards a series of curving lines. Similar wave patterns may become fan-like from development of crossing radial curves‡ (fig. 3).

Transverse bars, also curved, extend between straight axes, where these are near together, and where the fine material is abundant in somewhat granular substances§ (fig. 2). The predominance of radial or of concentric or wave pattern is mainly due to the form of the area and mode of retreat, since they are shown in the same material. Feather forms are developed within confined areas, as in ovoid bubbles which are contained in the margin of a deposit of vandyke brown (fig. 4).

This example (fig. 4) also illustrates other forms. The mud dried, or partially dried, along part of the edge and water from the included mass then tried to escape. Thus, bubbles were formed, generally elliptical or ovoid, and elongated transversely to the

* Gamboge, Upper Headon marl, boulder clay, pipe-clay, some slides of chalk, a tendency in vandyke brown, sepia, vermilion, &c.

† This was shown in one slide of vandyke brown. In another slide of that material the cover was not sealed, and although the mud caked at part of the edge (the part figured in fig. 4), the water escaped along most of the sides, and straight axes were produced.

‡ Vandyke brown, Prussian blue, crimson lake, ultramarine, vermilion, sepia, chalk.

§ Like chalk, pipe-clay, &c; not smooth uniform substances, as vandyke brown, gamboge, &c.

edge, like vesicles towards the surface of an igneous sheet or dyke. Still more interesting are tapering pointed canals, some of which radiate from the bubbles or from a central point. Others are generally branched, often slightly curved, and have a central granular streak. Near the sealed edge, canals are numerous and vesicles are large and few; while beyond this part, the canals diminish in number and finally almost entirely give place to numerous but small vesicles. The intervening mud exhibits various contraction cracks which originated in drying.

Thus, the structures may be classified as :—

1st. Those of coarser threads,

(1) where these form a maze;

(2) where they produce straight axes usually with terminal (or initial) knobs;

2nd, the finer pattern,

(3) consisting of concentric or wave-lines;

(4) forming feathery or fan-shaped tufts;

3rd (5) vesicular structures, including rounded vesicles, or tapering canals;

4th (6) contraction structures, cracks.

This classification is somewhat artificial and intended to indicate the chief types, between which gradations may be found; and by combination and aggregation of different forms numerous varieties arise, as may be seen in the few figures reproduced.

3. Conditions and Causes of Formation.

It is not always easy to describe concisely the exact conditions that govern the formations since they include at least three variables. First, the nature of the material, its specific gravity, adhesive character, and the size of the grains; second, the relative amount of the material, *i.e.* the muddiness of the fluid mass; third, the form of the area occupied and the direction of retreat during subsequent drying. As an example of the influence of the last-named condition: in one case, when indigo was carried in by a current, the coarser particles were deposited over a triangular or semicircular area, while the finer material spread over the whole space.* Further, any pressure or force acting upon the mass during drying may influence the form assumed; thus bubbles sometimes exercise a resistance on the part surrounding.†

Materials which consist of grains too large and heavy to be easily moved may give rise to a rude attempt at a pattern. Thus, ordinary fine sand exhibited a dendritic form, a deep channel or gutter extend-

* The corner of the cover-glass was propped up with a few bristles.

† Included bubbles sometimes are surrounded by concentric bands of the finer material, as if the bubble favoured a uniform evaporation.

ing as an axis, and shallower narrower furrows forming the branches.

In materials which consist of very finely divided particles, the variation in the size of the grains is too slight to produce any effect and the result is a uniform pattern. Thus indigo generally forms a typical example of a maze. If, however, the material is smooth and oily, like gamboge, it is very slow in drying, and the general form which it assumes is that of knobbed straight axes.

In powders of medium grain the difference in the size of the particles causes marked effects (*e.g.*, chalk, prussian blue, sepia, &c.) If the mud is dense, drying uniformly, a mazy network is usually formed. A thinner mixture may produce knobbed straight axes. An intermediate condition in the drying mass gives rise to feathery or fan shapes, or to parallel generally curving bars.

The mode of formation can be understood by tracing successive stages, and by comparing different examples. When the water begins to evaporate, the retreating edge of the film drags back the coarser particles, leaving behind some of the finer material in a moist condition. This afterwards dries, not however quite uniformly, so that it is ranged in wave-like lines, between which clear spaces or uniform thinner material may intervene. If the boundary is wide the lines will be almost straight, if the margin is narrow they will curve with smaller radius. The film of finer material, after retreating, sometimes advances again in a slightly different direction as was seen in watching it, and two sets of curving lines are thus formed, which partly cross one another.

Meanwhile as the thick film retreats, its rim becomes concave between certain points. Then the boundaries of adjacent concavities approach, and in this way the intervening material is gradually reduced to a streak. Inequalities, however, exist within the film, and, where coarser grains occur, or a clot adheres closer to the glass, these scrape away the finer material, thus clearing paths and leaving intervening streaks, which are radial, often curved and transverse to wave-lines. The clearer furrows may terminate at a knot of coarser grains, when the force no longer suffices to move these back, so that they remain isolated or form the initial knob of a thick stem. This may extend as a straight axis, or, if the film of air broadens as it indents and pushes back the muddy fluid, the curving margin may form the beginning of a maze or network, such as was described above.

Between the thick axes fine stuff is distributed on similar principles to those which caused the forms near the edge. The two dominant structures are the nearly parallel or concentric wave-lines and the radial streaks. But in this case the finer material is *deposited within* limited areas by which the forms are modified. If

the wave-lines only are strongly marked, they may extend as curving transverse bars; if the radial streaks are the dominant feature they form radiating curves or feathery or fan structures. A thick mass in its final desiccation may show cracking like the hexagonal flakes of mud at the edge of a pond, or the thick axes of a slowly drying material (like vermilion) may be jointed transversely. Sometimes stellate cracks may form within a mass, as in the example already described (fig. 4).

4. Possible Applications.

We have now to consider the possible applications of these simple experiments to structures which occur in Nature. Water, carrying fine suspended material, will often penetrate into rocks along a plane of discontinuity, and drying may then give rise to a "pattern."*

1stly. Dendritic forms upon joint or other surfaces are generally regarded as the results of crystallisation, and this undoubtedly is often the case, as frost spreads on a window pane. But if the formation takes place under the constraint of a narrow space, it may be caused simply by desiccation in the way described above. It will not be easy in all cases to infer which has been the exact mode of origin; sometimes the angle at which the branches diverge in the dendritic structure will prove that the formation was governed by crystallising forces; but it is clear that both conditions often may co-operate.†

2ndly. If no cavity exists along the plane of weakness, and the coarser axes are adherent and imbedded within the mass, they might appear to represent tubular structures—to be, as it were, flattened cylinders. In course of time the molecular and the mineral character might be changed, and crystallisation even might take place either in the original or the replacing substance. Is it not possible that this may be the explanation of certain puzzling structures sometimes placed as doubtfully organic, such as some of the so-called "fucoid" markings, or the peculiar forms in certain limestones? ‡

3rdly. These last speculations lead us to consider the possibility of similar contractions taking place in space of three dimensions, as we may call it, instead of two—in the mass of a rock, instead of along its surface. Thus the principle governing the formation of the landscape marble, as described by Mr. B. Thompson,§ presents some

* In some experiments I used surfaces of rock, and obtained results on a slab of London clay, of slate, &c., only, as might be anticipated, the forms were coarser and less regular than on smooth glass.

† See note by Professor Bonney appended.

‡ This is not intended to refer to the *Girvanella* forms described by Mr. Wethered ('Quart. Journ. Geol. Soc.,' 1890 to 1895, vol. 46, p. 270; vol. 47, p. 550; vol. 48, p. 377; vol. 49, p. 236; vol. 51, p. 196) nor of necessity to *Eozoon*.

§ 'Quart. Journ. Geol. Soc.,' 1894, vol. 50, pp. 393—410.

similarity, although there it is the rise of gas hindered by material instead of the lateral spread of air, causing the retreat of the water-film which opposes it. Many peculiar forms assumed by concretions may be similarly explained.

The deposit of an opaline layer with cusped points around a cavity might be caused by the uniform drying of a smooth, homogeneous material. Modified by crystallisation, it might help in the formation of agates and chalcedonic deposits.*

Igneous rocks have formed from a molten mass which may generally be considered (as pointed out by Lagorio) like a solution of the less fusible in the more fusible constituents, or in some cases may consist actually of two magmas imperfectly mixed. As it becomes solid the two parts may mutually react in a manner analogous to the phenomena already discussed. The forms shown in the secondary silicification of certain rocks (*e.g.*, cherts, silicified rhyolites, &c.) perhaps afford another illustration. Though this process is not exactly comparable with the deposits of sediment described above, yet colloid silica might penetrate in different directions, and encountering constant obstacles, give rise to an irregular, almost felsitic structure.

4thly. In the specimens which show cracking and expansion of cracks to canals† or bubbles, we may see a resemblance to other forms in igneous rocks (fig. 4). Some of the stellate or cruciform cracks remind us of the kind of contraction which, in a semi-solidified pyromeride, develops fissures and inlets, afterwards filled by chalcedonic or other deposits. Further, the grouping of cracks and bubbles in the drying film of mud, imitating roughly some of those found within spherulites, would suggest that in certain of these a crust may be formed before their complete development. This would accord with the view which Professor Bonney has often advanced, in conversation with me, for certain Boulay Bay pyromerides,‡ and with the hypothesis which seemed to be suggested by certain Welsh pyromerides and variolites: § that the spherulite sometimes follows, as it were, an initial formation of a nodule which has arisen by flow-brecciation, or other process.

Thus it seems possible that in several points the simple forms I

* A comparison similar to that with the landscape marble might be made with the process for the artificial formation of agates described by Messrs. J. I'Anson and E. A. Pankhurst, 'Min. Mag.,' 1882, vol. 5, p. 34.

† Compare with these the branched canals in gelatin described by Professor Sollas, as due to the formation of ice spicules ('Trans. Roy. Irish Acad.,' 1890, vol. 29, p. 427).

‡ I believe that this view has the support of Mr. J. Parkinson, F.G.S., but I leave the sentence as it was written about two years ago (see 'Quart. Journ. Geol. Soc.,' 1898, vol. 54, p. 101).

§ 'Quart. Journ. Geol. Soc.,' 1889, vol. 45, p. 268; and 1893, vol. 49, p. 152.

have tried to record may help to throw light on certain structures and processes in rocks. Applying the principles we have noticed here, we might anticipate (even where crystallising materials are present)—

1stly. That mutual interactions may give rise to wriggling or mazy forms.

2ndly. That greater freedom of molecular motion might cause more rectilinear forms.

3rdly. That the relative ease of transmission in different substances or conditions of a substance may govern the forms developed, as the difference between the coarser and finer sediment.

4thly. That the form of the external boundary influences the structures developed, so that streaks normal to a rectilinear figure, or radial within a sphere, are caused.

5thly. That much depends on the relative permeability of the surrounding magma; and the solidification in a molten mass, first of an external crust, would act similarly to a more impenetrable environment.

PART II.

To have fully discussed experiments in which any effect of crystallising forces is shown would have unduly lengthened this paper, and on that subject some material has been published.* My main object is to call attention to the results of mere mechanical rearrangement, on which I think no distinct notice is available. But I may briefly refer to a few results of crystallisation. I experimented with solutions of various salts, mixed with gelatin, with "muds" of vermilion, &c., and in other combinations.

When solutions of calcium sulphate and of gelatin were mixed in varying proportions, if the solution was weak, crystallisation generally seemed to start at many scattered points. At these there formed small crystals or ovoid grains (like potato starch-grains, often compound) or clusters, frequently spherulitic. In the intervals was a gelatinous-looking deposit, which had no effect on polarised light.

* H. Vater, in 'Zeits. für Kryst. und Min.,' 1892-6, vol. 21, p. 433; vol. 22, p. 209; vol. 24, p. 366; vol. 27, p. 477. O. Lehmann, in 'Zeits. für Kryst. und Min.,' 1877, vol. 1, p. 453. 'Quart. Journ. Micr. Sc.,' 1855, vol. 3, p. 179, Pl. 13, 14; and 1856, vol. 4, p. 203, Pl. 12, by J. Glaisher; vol. 4, p. 201, by J. Spencer; 1861, vol. 1, n.s., p. 23, by G. Rainey; 1862, vol. 2, p. 128, by T. Davies; 1866, vol. 6, p. 137, by R. Thomas; 1872, vol. 12, p. 118, by Professor Harting. 'Roy. Soc. Proc.,' 1866, vol. 15, p. 314, by E. Montgomery. 'Trans. Brit. Assoc.,' 1867, p. 127, by Dr. Heaton. 'Phil. Mag.,' 1878, vol. 6, p. 113, by F. Guthrie. 'Intellectual Observer,' 1865, vol. 6, by H. N. Draper. 'Trans. Roy. Micr. Soc.,' 1871, p. 50, by H. S. Slack. 'On the Influence of Colloids upon Crystalline Form, &c.,' by Dr. W. M. Ord, 1879. 'Nature,' 1892, vol. 47, p. 162, by Dr. J. K. Gladstone.

As we add a larger amount of the salt solution, the following effects were produced: first, the crystallised bodies all became larger; more crystal-shaped grains formed, instead of the rounded blebs,* and then, beyond an irregular, clear space around each, the ground was occupied by a more distinctly dendritic deposit, which faintly depolarised. In a more watery medium, the ovoid grains are more marked. From a mixture of vermilion in a solution of the salt, the results are similar, but the vermilion is in clots or lumps, or scattered granules, with intervening, irregularly shaped spaces. In one slide the clear spaces are rather definitely shaped and angular, almost as if dominated by crystallising force, yet the crystallising salt has formed only small, ovoid grains, clustered rather more thickly near the central part of the space.

The formation of spherulitic structures has been described by several authors. Good examples were obtained in these experiments with sodium phosphate mixed with gelatin. A weak solution, or a solution of the salt without the gelatin, gave spherulitic structures, though these were small and scattered. From a stronger mixed solution the deposit consisted of rounded, gelatinous-looking globules, more or less aggregated, which, with crossed nicols, showed radiate structure and a black cross. Radial tufts of acicular crystals of calcium sulphate are well known among microchemical tests;† these were obtained from pure solutions of the salt, or from mixtures with "mud," or with gelatin. One small isolated drop ($\frac{1}{8}$ inch across) of a mixture of calcium sulphate solution and vermilion, which dried on another slide,‡ exhibited spherulitic spheres or hemispheres (their centres being on, or just within, the circumference of the drop), clustered like those which have been obtained in a sheet of glass which had been raised to a high temperature and then cooled.§

In other drops, where definite crystals were formed, the crystals projected inwards, often with branched, almost dendritic, or skeleton, growth.|| If in certain spherulites a radial ingrowth of crystallites takes place, as seems probable, it would be somewhat analogous to this result of shrinkage or skin tension in the drop.

In certain examples, where a rather concentrated solution was

* Cf. H. Vater, 'Zeits. für Kryst.,' vol. 27, p. 439.

† "Notes on the Micro-chemical Analysis of Rock-making Minerals," by Lieut.-General C. A. McMahon, F.G.S. ('Min. Mag.,' vol. 10, p. 110). 'Manual of Micro-Chem. Anal.,' Professor H. Behrens, p. 71.

‡ I failed to reproduce this structure again, although many variations in the conditions were tried.

§ "Address of President," by Professor T. G. Bonney, 'Quart. Journ. Geol. Soc.,' 1885, vol. 41, p. 63. 'Roy. Soc. Proc.,' 1885, vol. 39, p. 103, by D. Hermann and F. Rutley.

|| For the mode of growth and general form of the crystals, compare Lehmann in 'Zeits. für Kryst.,' 1877, vol. 1, Pl. 21, figs. 49, 50a Pl. 22, fig. 84.

mixed with material in suspension (e.g., calcium sulphate and vermilion), a mass of crystalline grains, separated by clear, narrow interspaces, occupy a definitely crystal-shaped area. The neighbouring grains are related in form, the spaces between are bent or curved, the whole resembling micropegmatitic or pegmatitic structure (fig. 5.)

In the formation of chiasolite and other secondary minerals, the matrix is often partly included, but in these experiments the more fluid medium apparently separated from the vermilion, thrust it to the edge of an initial, crystal-shaped area, and within that formed crystalline grains separated by interspaces. The grains generally depolarise uniformly, but occasionally are built up of clustered prisms, and sometimes even of slender, tufted needles. Lehmann, in his classic paper, describes the causes of irregular, or interrupted growth as due mainly to the viscosity of the medium or the presence of foreign substance*. He points to spherulitic and dendritic forms as results. The micropegmatite seems to be one more possible development.† This would agree with the hypothesis advanced by Professor Bonney, that it results in igneous rocks, when the magma is kept at a somewhat persistent, but not too high, temperature; so that the material probably would be in a very viscous or partly solidified condition.‡

While this paper was in progress, I received from Professor Bonney the following interesting note (drawn up by him some years ago),§ the more valuable as made upon one of nature's experiments. The observation adds one more suggestion as to the possible formation of "pseud-organic" structures, that they might originate from a mixture of mechanical sediment and of a crystallising salt, and the forms in the mud might remain even if the salt were afterwards dissolved.

The note is as follows:—"In walking along the pavements during the late frost, before the sun or the feet of the public had produced an effect, I was often struck with the forms of the ice crystals. The pavements were dirty; much fine mud, brought from the roads on the boots of pedestrians, had been pretty evenly distributed in a thin film. During the night this had been arranged in rod-like crystals, often 3 or 4 inches long. These formed groups, spreading like the sticks of a partly opened fan. They were sometimes

* 'Zeits. für Kryst.,' 1877, vol. 1, p. 453.

† Micropegmatite is closely related to the other forms.

‡ "On a Contact Structure in the Syenite of Bradgate Park," 'Quart. Journ. Geol. Soc.,' 1891, vol. 47, p. 107.

§ Of this note, written in December, 1892, an abstract was sent to 'Nature' (December 15, 1892) by the author in corroboration of a letter, which had appeared the previous week, from Professor Meldola, calling attention to the same phenomenon. Several letters (from Dr. J. H. Gladstone and others) on the same subject were printed at the same time. 'Nature,' vol. 47, pp. 125, 1892.

slightly curved,* perhaps about $\frac{1}{2}$ inch in diameter, and $\frac{1}{2}$ inch at thickest. They differed from the 'frost ferns,' in a much greater simplicity of structure, being more like little bunches of grass arranged along a stem than those exquisite and intricate fabrics of the ice-world. They reminded me sometimes of the groups of actinolite crystals in certain crystalline schists, as for instance at the St. Gothard. These actinolites, on examination under the microscope, prove to be not pure crystals, but much interspersed with granules of pre-existing minerals. It occurred to me that the probable reason for the formation of these ice structures was the impediment offered by the small grains in the mud to the growth of tiny crystals. Only here and there, where circumstances were exceptionally favourable, a crystal larger than usual would be developed, which, as it advanced, gathered to itself other crystalline molecules. Thus would be started a bunch of coarse crystals, springing from a common centre, growing more easily in the direction of the axis of the rhombohedron, just as in the case of frost ferns, but they would be simpler, coarser, and arrested by the impediments, instead of delicately flexured by the almost imperceptible inequalities of the glass.

"I obtained on a later morning, strong confirmation of this view. Heavy rain fell all through the preceding day, followed by a clear night, with a ground-frost. In many places the pavements had been washed very clean. On the *clean* paving stones I saw, not unfrequently, early the next morning, fairly delicate frost ferns (though not equal to those on glass). Where the surface had been a little dirty, the forms were less intricate and coarser; in some rather dirty places the old types could be seen.

"*Postscript.*—What is written here was my impression at the time, but I have since doubted whether the crystals were not 'muddier' than the part around. It was not possible to examine the pavement very closely; also I noticed on a later occasion that the trampling of the film of mud (prior to freezing), which produced a kind of concentration in an irregular network of wavy lines or bands, appeared to have something to do with the formation of the flowers."

This note suggests that analogous processes may be traced in schists, like those of the St. Gothard, and the mode of formation of certain crystals in schists has been described in papers already published.† On this point, however, it is needless to enlarge, as we may hope to receive more results, since Professor Bonney has communicated a paper dealing with this subject to the Geological Society.

* Similar forms were noticed on certain days in the winter, 1896-7.

† "On a Secondary Development of Biotite and of Hornblende in Crystalline Schists from the Binnenthal," by Professor T. G. Bonney, 'Quart. Journ. Geol. Soc.,' 1893, vol. 49, p. 104. Also "On some Schistose Greenstones, &c.," by the same author, *ibid.*, p. 94.

FIG. 3.



FIG. 2.

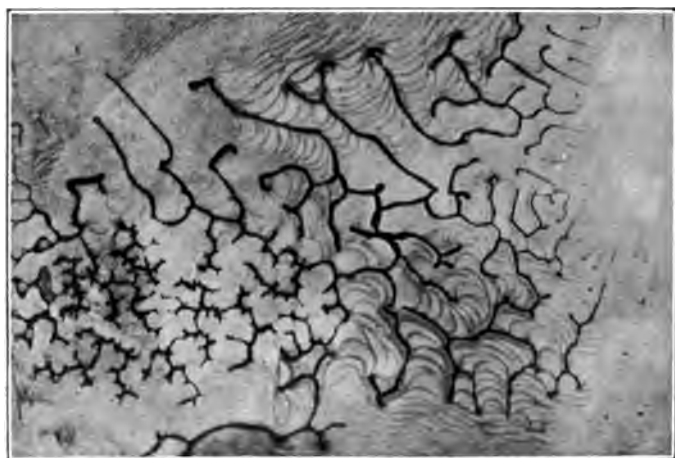


FIG. 1.

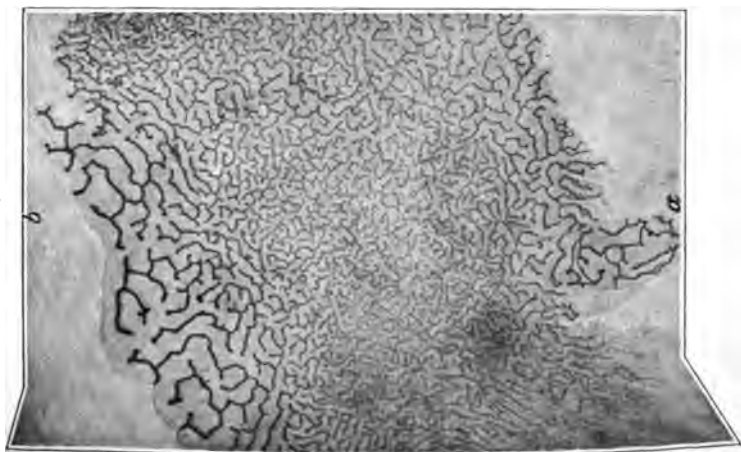
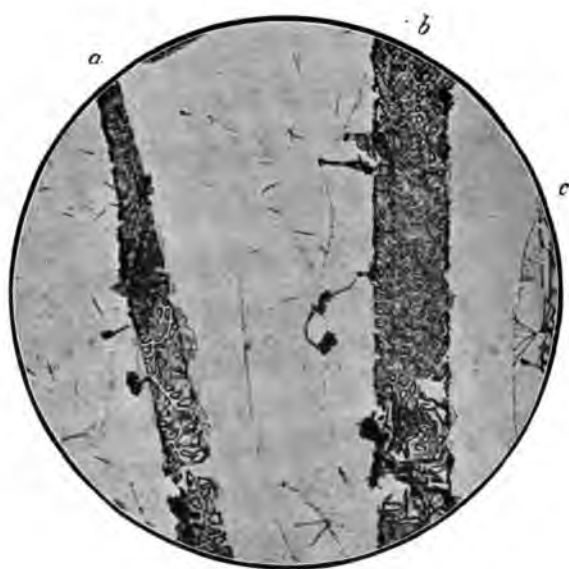


FIG. 4.



FIG. 5.



EXPLANATION OF FIGURES (PLATE 2).

The results shown in figs 1—4 were obtained on glass plates, about 6 inches by 3 inches, with cover-glasses about 3 inches by 2 inches. The whole pattern is represented in figs. 1—3, but fig. 4 shows a small part only of one slide (magnified). In each experiment a drop of the mud was placed on the slide, and then covered, so that the mud spread out somewhat irregularly. In other examples similar results were obtained, after running in material or keeping up a current for a short time to carry the mud along.

FIG. 1.

Prussian blue in water. This was placed on the glass as a large drop, and a very small one was accidentally deposited near by, in which the pattern at *a* has been developed. The mud in a few minutes began to show indication of the future pattern. Very shortly the film retreated from two edges (*a* and *b*), where the mud was "denser," and where it dried to a coarse maze. Afterwards the fine maze formed, and was completely developed in two or three days.

FIG. 2.

Chalk mass, somewhat thin; tilted so that the cloudy chalk gradually flowed down. In this pattern the rods tend to become straighter, connected by curved transverse bars.

(A pattern from denser chalk in several cases, not figured, was that of a coarse maze.)

FIG. 3.

Prussian blue. The fine pattern near one edge (*a*) with concentric or wave-lines, and some clearer radial furrows was first developed. Then coarser bent stems were deposited, and towards the further margin these became smaller, interrupted, and finally reduced to isolated spots, while the fine material formed rather feathery tufts (less distinct in the photograph). Dried in about two days.

FIG. 4 ($\times 8$ diameters).

From a large slide of vandyke brown, which formed straight axes and a fine "wave" pattern (an indication of this is shown at one side of the figure). Part of the edge "caked" in drying, and within it, bubbles developed, often elongated transverse to the margin, and stellate cracks or series of cracks. Fine material, somewhat faintly marked in the figure, has formed roughly oval-shaped patches in the vesicles or a central streak in the cracks.

Note.—Certain sharp lines, partly overlapping the pattern in figs. 3 and 4, mark the edge of a film of canada balsam, by which the cover was sealed down.

FIG. 5 ($\times 25$ diameters).

Mixture of a solution of calcium sulphate and vermilion dried on an ordinary microscope slide beneath a cover-glass.

The figure represents parts of two long skeleton crystals of calcium sulphate (*a* and *b*) in which a micropegmatitic structure has formed. A small part of a third crystal (*c*) is shown, the substance of which extends more continuously. The vermilion partly borders the edges of the large skeleton crystals, and is black and opaque in the figure. Other black patches in it are composed of aggregated crystallites of calcium sulphate with some vermilion. The fine scattered lines or radial tufts are similar crystalline needles of the salt.

"The Relations between the Hybrid and Parent Forms of Echinoid Larvæ." By H. M. VERNON, M.A., M.B. Communicated by Professor RAY LANKESTER, F.R.S. Received March 29,—Read May 5, 1898.

(From the Zoological Station, Naples.)

(Abstract.)

The object of this research was to determine systematically during a period of several months' duration, the exact relationship of structure and size existing between certain hybrid and parent Echinoid larval forms. Eight different species of Echinoids were worked with, but the larger number of observations were confined to three of them, viz., *Strongylocentrotus lividus*, *Sphærechinus granularis*, and *Echinus microtuberculatus*. The method of procedure was similar to that described in a former paper.* It consisted in shaking pieces of the ovaries and testes of the various Echinoids in small jars of water, and then mixing portions of the contents. In the cross-fertilisations, precautions were of course taken to prevent any accidental direct fertilisation of the ova. After standing an hour, the now fertilised ova were transferred to large jars of water, holding, as a rule, 2 to 3½ litres. Here they were allowed to develop for eight days, when the plutei formed were killed, preserved, and examined under the microscope. The structure of the hybrids, in relation to that of the parent forms, was studied; and all the larvæ, both pure and hybrid, were measured by means of a micrometer eye-piece in respect of their body length and anal arm length. Fifty larvæ were, as a rule, measured in each case. In addition, the ova were examined under the microscope twenty-four hours after fertilisation, and the numbers of blastulæ and unfertilised ova in a given volume of water counted. Again, after eight days, the number of plutei surviving was similarly estimated.

Upon the cross *Sphærechinus* ♀ *Strongylocentrotus* ♂, twenty-two experiments were made. As a rule only about 10 per cent. of the ova were fertilised, and only 1 per cent. of them reached the eight days pluteus stage. The hybrids were most easily obtained in the summer months, few or none of the ova being cross-fertilised in the winter, unless the aids to fertilisation made use of by O. and R. Hertwig,† and by Born,‡ were adopted. Thus the former observers showed that if the ova were shaken in water and kept some hours, so that their vitality became diminished, they underwent cross-fertilisation

* 'Phil. Trans.,' B (1895), p. 577.

† 'Jenaische Zeitschrift f. Medicin,' vol. 19, p. 121 (1886).

‡ 'Pflüger's Archiv,' vol. 32, p. 453.

much more readily than when freshly shed. Born, on the other hand, found the cross fertilisation could be increased by increasing the amount of sperm added. Both methods, but especially the former, were found of value in the present research.

As regards the structure of the hybrids under discussion, it was found that the majority of those obtained in May, June, and July were of an almost pure *Sphærechinus* type, only a third or less of them being of an intermediate or *Strongylocentrotus* type. In November, on the other hand, only about a sixth were of the maternal, and five-sixths of the paternal type. Finally, in December and January, all the hybrid larvæ were of the paternal type. These latter larvæ in almost all cases showed obvious traces of their hybrid origin, but they were evidently much more inclined to the *Strongylocentrotus* than to the *Sphærechinus* type. Thus the body skeleton was like that of *Strongylocentrotus*, only it generally had a few abnormal projections springing from it. It was also, as a rule, about 25 per cent. shorter. The anal arm skeleton generally consisted of two rods, but there were very seldom any cross bars joining them, such as occur in *Sphærechinus* larvæ.

On the reciprocal cross of *Strongylocentrotus* ♀ and *Sphærechinus* ♂ eighteen experiments were made. During April, May, and June a fair number of the ova were cross-fertilised, but no plutei were obtained. In July and August some 47 per cent. of the ova were fertilised, and 29 per cent. of them survived to the eight days pluteus stage. In November and December, on the other hand, with one exception, not only were no plutei obtained, but as a rule not a single ovum was cross-fertilised. The hybrid larvæ themselves were of the pure *Strongylocentrotus* type, but in one instance the arms of the larvæ were very much longer than had ever been noticed in any other case. These hybrids appeared therefore to be of the nature of sport.

These extraordinary variations in the capacity for cross-fertilisation seem to be due to the variations in maturity which the sexual products undergo with change of season. Thus in the summer months most of the *Strongylocentrotus* individuals contain but very small quantities of ripe sexual products, or none at all. Also from the fifty different series of observations made on normal *Strongylocentrotus* larvæ, it appeared that the size of the larvæ kept at the maximum from the beginning of April till the beginning of May, but then began to dwindle down, so that at the beginning of July it reached its lowest level. The larvæ were now sometimes 30 per cent. smaller than the spring larvæ. After the middle of August they gradually increased again, and by the end of November had attained their maximum size. *Sphærechinus* larvæ, on the other hand, kept at about the same size throughout the year, though considerably

230 *Relations of Hybrid and Parent Forms of Echinoid Larvæ.*

smaller numbers of the ova reached the pluteus stage in the summer than in the winter months. We see therefore that the *Strongylocentrotus* ♀-*Sphærechinus* ♂ hybrid is only formed at the time when the *Strongylocentrotus* ova have reached their minimum of maturity; whilst in the case of the reciprocal hybrid, it follows that as the maturity of the *Strongylocentrotus* sperm increases, it is able to transmute first a portion and then the whole of the hybrid larvæ from the *Sphærechinus* to its own type. In other words, the characteristics of the hybrid offspring depend directly on the relative degrees of maturity of the sexual products.

As a result of the ten experiments made on the cross *Echinus* ♀-*Strongylocentrotus* ♂, it was found that the hybrid larvæ were on an average about 8 per cent. larger than the pure parental larval forms, and, moreover, that even more of the cross-fertilised ova developed to plutei than of the directly fertilised ones. The hybrid larvæ were of a variable, but more or less intermediate, type. In the reciprocal cross, on the other hand, only a small proportion of the ova underwent fertilisation, and only about 1 per cent. of them reached the pluteus stage. These plutei were on an average 13·2 per cent. smaller than the pure maternal larvæ. In structure they were of a *Strongylocentrotus*-intermediate type.

Hybrids between *Sphærechinus* ♀ and *Echinus* ♂ were obtained on only two occasions. The larvæ were of a very variable *Echinus*-intermediate type, and were much dwarfed. In the reciprocal cross, hybrids were also obtained only twice, but then about 60 per cent. of the ova reached the pluteus stage. These hybrids were of the pure *Echinus* type. On most other occasions a small number of the ova were cross-fertilised, but failed to develop to plutei.

Hybrid larvæ, of the maternal type, were obtained on crossing *Arbacia* ova with *Strongylocentrotus* and with *Echinus* sperm, but with *Sphærechinus* sperm only gastrulæ resulted. Hybrids were also obtained between *Arbacia* ♂ and *Strongylocentrotus*, *Sphærechinus*, and *Echinus* ♀. These hybrids were of the maternal types, but in some of the *Sphærechinus* hybrid larvæ the anal arm skeletons were similar to those in pure *Arbacia* larvæ.

Hybrid larvæ were obtained between *Echinocardium cordatum* ♀ and *Strongylocentrotus*, *Sphærechinus*, *Echinus*, and *Arbacia* ♂, the larvæ being of the maternal type, but somewhat modified by the nature of the sperm. The hybrids between *Echinocardium mediterraneum* ♀ and *Strongylocentrotus* and *Echinus* ♂ were of an intermediate type; hence, one is afforded a physiological argument in favour of the specific difference of these two forms, the existence of which has hitherto been considered rather doubtful.

Hybrid larvæ of the maternal type were obtained on crossing *Strongylocentrotus* ♀ with *Dorocidaris* ♂, but the reciprocal cross

yielded only gastrulæ. Finally, hybrida, of a presumably intermediate character, were obtained from the cross *Echinus microtuberculatus* ♀ and *Echinus acutus* ♂. With the ova of *Sphærechinus* and *Echinocardium*, and the sperm of *Echinus acutus*, only gastrulæ were obtained.

On performing cross-fertilisations with the colour varieties of *Sphærechinus*, there was found to be a distinct diminution of fertility. In the most marked instance, obtained in the experiments made on June 2, it was found that when white-spined varieties were fertilised with white-spined, and violet-spined with violet, 98·5 per cent. of the ova reached the blastula stage, and 73 per cent. the eight days pluteus stage. But on cross-fertilising white-spined with violet-spined individuals, only 68 per cent. of the ova developed to blastulæ, and 15·6 per cent. to plutei. Also these crossed larvæ were 4·5 per cent. smaller than the uncrossed. Other series of experiments were made in July, November, and December, the differential fertility seeming to gradually diminish with the progress of the season. Nevertheless, it was always most distinctly present. On crossing the less definitely marked colour varieties of *Strongylocentrotus*, a small amount of infertility seemed to be present in one series of experiments, but none at all in another.

May 12, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Professor Dewar made a preliminary communication "On the Liquefaction of Hydrogen and Helium."

He prefaced his statement by referring to a letter which he had addressed to the President on the 10th May, announcing to him the fact that he had succeeded in liquefying hydrogen in quantity, and that by means of the liquid hydrogen he had also liquefied helium.

The following Papers were also read:—

- I. "On the Magnetic Susceptibility of Liquid Oxygen." By Professor FLEMING, F.R.S., and Professor JAMES DEWAR, F.R.S.
- II. "A Study of the Phyto-Plankton of the Atlantic." By G. MURRAY, F.R.S., and V. H. BLACKMAN.
- III. "The Electric Response of Nerve to a single Stimulus investigated with the Capillary Electrometer. Preliminary Communication." By Professor GOTCH, F.R.S., and G. J. BURCK.

IV. "Effects of prolonged Heating on the Magnetic Properties of Iron." By S. R. ROGET. Communicated by Professor EWING, F.R.S.

V. "On the Connection of Algebraic Functions with Automorphic Functions." By E. T. WHITTAKER. Communicated by Professor FORSYTH, F.R.S.

The Society adjourned over Ascension Day to Thursday, May 26.

"A Calorimeter for the Human Body." By WILLIAM MARCET, M.D., F.R.S. Received March 10,—Read April 28, 1898.

(From the Physiological Laboratory, University College, London.)

At the meeting of the Physiological Society held at University College in March, 1897, I exhibited and described a calorimeter constructed for the purpose of determining the heat given out by man. Several members of the Society, in succession, allowed themselves to be shut up in the chamber where they experienced no discomfort whatever. The instrument was also described the same year to the Société de Physique et d'Histoire Naturelle of Geneva, but no full account of it has been published so far.

The first calorimeter for the investigation of animal heat was made by Lavoisier and Laplace,* who enclosed an animal in a chamber surrounded with ice and determined the heat evolved by measuring the amount of ice melted. Crawford, in 1788, placed the air chamber inside a water-jacket, and determined the heat emitted by means of the increased temperature of the water. An objection to this type of calorimeter is the very small rise in the water temperature, and the difficulty of obtaining an uniform temperature in such a large volume of water. J. Rosenthal,† in 1878, introduced a calorimeter in which the heat given out from a small animal was absorbed by a fluid with a low boiling point, such as ordinary ether, the amount of heat was calculated from the volume of the fluid evaporated and its known latent heat of vaporization.

Rosenthal,‡ at a later date, constructed a calorimeter, which consisted of three concentric chambers of sheet copper, and was made in duplicate; the two instruments were connected by means of a U-shaped manometer. The heat given out by an animal, such as a dog, enclosed in the innermost chamber of one of the instruments,

* 'Mémoires de l'Acad. des Sciences,' 1780.

† 'Archiv f. Anat. u. Physiol.' (Physiol. Abthg.), 1878, p. 349.

‡ C. Rosenthal, 'Arch. f. Anat. u. Physiol.' (Physiol. Abthg.), 1888, p. 1; J. Rosenthal, *ibid.*, 1889, pp. 1, 23, 39.

was communicated to the middle chamber, and from the position of the meniscus in the manometer, the initial temperature of the experiment and the barometric pressure, the heat emitted in a given time was indirectly calculated.

Experiments were made on man with a modified form of this instrument by enclosing an arm in one of the two inner chambers. Hirn* investigated the heat emitted by man by making use of a wooden chamber in which he had previously ascertained the rate of loss of heat through its walls. This was done by burning a known volume of hydrogen gas within the chamber until the temperature became constant, and then determining the heat lost per unit of time, from the known volume of gas burnt. This method at first sight commended itself by its simplicity, but was open to two objections—the first that it was an indirect method of inquiry, the second that the person under experiment was unavoidably subjected to a high temperature.

In 1889 Richet† published an elaborate investigation on animal heat, in which he made use of a calorimeter constructed in such a way that the heat emitted by an animal in a closed vessel displaced by pressure a volume of water equal to the expansion of the air in the calorimeter.

An ingenious animal-calorimeter was constructed lately by Messrs. J. S. Haldane, W. Hale White, and J. W. Washbourne.‡ These gentlemen determined the heat given out from an animal, by comparing the pressure resulting from the expansion of the air in a closed jacketed space surrounding the chamber containing the animal, with the corresponding expansion produced by the burning of a known volume of hydrogen gas in another similar vessel. The amount of gas burnt is regulated with a stopcock, so that its heat should correspond exactly with that produced in the other chamber as indicated by a differential manometer. The heat emitted is equal to that of the combustion of the gas burnt.

Messrs. W. O. Atwater and E. B. Rosa, of Connecticut, U.S.,§ have quite recently measured the heat emitted from a person by placing him in a large calorimeter, where he lived for periods of from one to twelve days. The walls of the chamber were double, and made of sheet copper and sheet zinc, while the heat generated was

* "La Thermodynamique et l'étude du travail chez les êtres vivants," 'Revue Scientifique,' 1897. 'Recherches sur l'Équivalent mécanique de la Chaleur.' Colmar, 1858, pp. 51, 95. 'Exposition analytique et expérimentale de la Théorie mécanique de la Chaleur.' Paris, 1875, p. 35.

† "La Chaleur animale," par Ch. Richet, 'Bibl. Sc. Internat.,' 1889.

‡ 'The Journal of Physiology,' 1894, p. 123.

§ "An Apparatus for verifying the Law of Conservation of Energy in the Human Body," Brit. Assoc., 1897, Toronto, Trans. of Sec. A (General Physics).

carried away by a stream of water; according to the authors, tests showed this calorimeter to be very accurate.

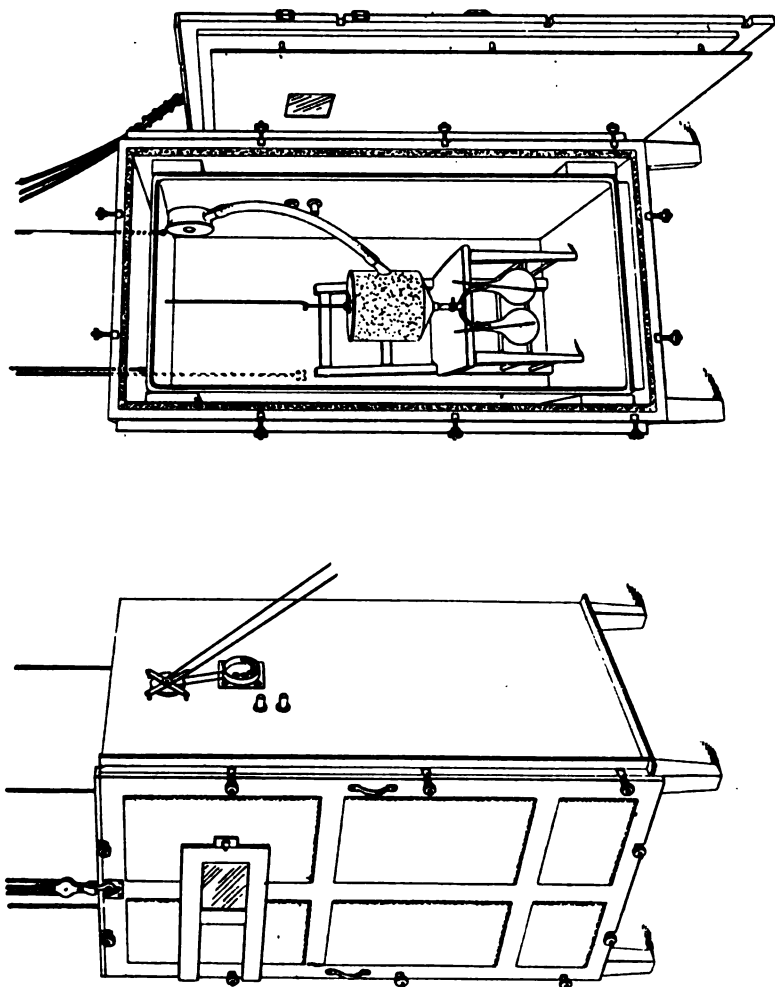
The construction of the present instrument was suggested from Berthelot's calorimeter, for the determination of specific heat by mixtures, in which the heat given out is reflected by bright surfaces of silvered copper surrounding at a short distance the vessel containing the mixture. It was found, however, in experimenting with the new calorimeter (which was not silvered inside), that all the heat was not reflected, a certain proportion being absorbed by the copper, which necessitated an arrangement for determining the temperature of the metal.

The instrument (see figure) consists of a wooden chamber lined internally with a thick padding of cotton wool, and externally with several thicknesses of felt. Inside this chamber there is another made entirely of sheet copper, the inner surface of which is maintained carefully polished; its height is 145 cm., and its breadth is 69 cm. The two chambers have between them an annular space from 4 to 5 cm. in breadth. The capacity of the copper chamber, empty, is 810.4 litres, and its weight 62.370 kilograms, therefore an alteration of 1° C. in the temperature of that mass of copper would be equal to 5832 (small) calories, or would raise 5832 grams of water 1° C.

The copper chamber is closed by means of a movable panel also constructed of copper, which is fixed to a wooden backing; the edge of the copper panel is made to press tightly against an india-rubber cushion carried round the rim of the opening in the copper chamber, while the edge of the wooden backing is applied against the rim of the wooden chamber, and the panel is kept in its place firmly with brass screws. This movable door is too heavy to be handled easily by one man, and on that account is fixed to a tackle fastened to a beam in the roof of the laboratory, by which means no difficulty is experienced in opening or closing the chamber.

There is a small window 21×15 cm., made of two superposed panes of very thick glass, and opening into the two chambers; when closed, the rim of the inner pane presses against a cushion around the corresponding opening in the copper chamber; it shuts by a spring bolt, and should the person under experiment feel uncomfortable, or wish to communicate with the outside, he can push open this window at any time.

Inside the copper chamber there are two ventilators, or perhaps more correctly "agitators," in the form of revolving fans, the object of which is to thoroughly mix the air inside the chamber. One of these agitators is fixed high up in the chamber, the other low down on the opposite side. The upper agitator was found experimentally to produce a blast of 190 litres per minute—say about 380 litres per



minute for the two; nearly the whole of the air in the chamber would be carried through the two agitators every two minutes.* The number of revolutions of the fans recorded by counters amounts together to two or three hundred thousand per hour.

The motor power for working the fans is obtained from the wires used for the electric light of the laboratory, and exerted through two small dynamos—one for each agitator, while the action of either one or the other can be regulated by a carbon-resistance.

* The resistance caused by the ice, and a rose jet on the track from the lower agitator, would probably reduce the draught to some little extent; no doubt, however, that the air is very thoroughly mixed inside the chamber.

The upper agitator is disposed in such a way as to drive the air of the chamber through a mass of ice roughly broken up, and held in a cylindrical tin vessel open at the top, suspended from the roof of the chamber. The cold air having from its increased density a tendency to fall, is taken up by the lower ventilator and driven upwards; by such means a circulation of the air in the chamber is maintained through the ice-holder. Should the temperature of the chamber rise during the experiment, by increasing the draught through the ice more ice is melted and the rise is checked; the reverse holds equally good.

The ice used for absorbing the heat emitted by the person under experiment delivers its water into a flask holding a thermometer divided into fiftieths of a degree centigrade; the flask and thermometer are both weighed. The flask hangs from a hook on the side of a tube projecting from the bottom of the ice-holder.

Besides the thermometer in the flask, there are three other thermometers all centigrade, also divided into fiftieths of a degree, connected with the calorimeter; the bulb of one of them projects into the copper chamber while its stem is carried outside above the wooden chamber; a second has its bulb fastened down to the side of the copper chamber by means of a strip of copper which covers it entirely, its stem also projecting outside; the third thermometer is passed through the wooden chamber into the annular space, whose temperature it shows during the experiment.

The successive stages of an experiment are as follows:—the heat emitted from the body is first rapidly distributed throughout the chamber, then it is absorbed by the mass of ice, reappearing in measurable form as water. Knowing that 79 calories are required for melting 1 gram of ice, the total calories corresponding to the ice melted are easy to calculate.

Some of the heat emitted falls upon the brightly polished surface of copper of the chamber, most of it is reflected into the chamber, but a certain proportion becomes absorbed in the metal, and is determined by the thermometer attached to the walls of the chamber. Perhaps a very small amount passes through the copper walls into the annular space, it might have been neglected, but has been taken into account in every experiment. A number of experiments showed that the copper was equally heated in every part, or very nearly so, while the test experiments made with hydrogen gas placed that question quite at rest.

The thermometers were generally read and the readings recorded every ten minutes, the temperature of the copper being used as a guide towards the maintenance of a constant temperature throughout the instrument.

It will be readily understood that a difference of as much as 1°

at the end of the experiment in the temperature of the air of the copper chamber was not of great moment from the low specific heat of air; indeed, such a difference would only yield 214 calories, which is but trifling on say, 90,000 calories given out in one hour. On the other hand, a slight difference in the temperature of the copper proved of importance on account of the mass of metal.

There was no difficulty, however, in maintaining the temperature of the copper within 0.3° or 0.4° of its original reading before the experiment. Should it accidentally run up beyond that figure, a very rare occurrence, this only lasted a minute or two, and by increasing the blast through the ice, the temperature of the copper was soon brought down to its initial reading. A similar remark applies to falling temperatures of the copper; by stopping the draught through the ice they soon rose to the initial reading. Of course, constant attention to the temperatures was required during the whole experiment, which, with but few exceptions, lasted one hour.

There remained, however, a serious difficulty to contend with, owing to the heat produced by the action of the ventilators or agitators.

At first the friction of the bearings on which the fans rotate was thought to be the main cause of this heat; and, in consequence, their position was altered so as to be placed entirely outside the wooden chamber. This, however, failed to mend matters, and it became evident that the friction of the revolving blades against the air was the source of the heat produced. The only method of overcoming the difficulty was to determine the heat produced exclusively by the agitators and subtract it from the total heat obtained in each experiment.

It was now found necessary to introduce counters registering the number of revolutions for each of the ventilators up to 1,000,000 turns. The inquiry necessitated by the ventilators (say agitators) took up a considerable portion of the winter 1896-97.

These experiments were carried out exactly in the same way as those made on the living body, with this difference, that while from 10 to 15 lbs. (4.5—6.8 kilograms) of ice were wanted when a human subject was under experiment, from 500 to 800 grams of ice only had to be used with the agitator experiments. The following is the result of one experiment taken at random amongst a great many others:—

	Chamber.	Annular space.	Copper.
Initial	15.59°	15.50°	15.35°
Final	15.55	15.52	15.50
	<hr/> -0.04	<hr/> +0.02	<hr/> +0.15

Weight of ice melted..... 97.46 grams.
 T. water from ice 10.92°

Calories recovered.

From melted ice.....	+7699
„ heat absorbed in water	+1064
„ „ of air in chamber	-9
„ „ air in annular space ..	+1
„ „ copper (of chamber) ..	+ 875
	<hr/>
	9630
	(9 subtracted)

Number of revolutions of agitators..... 342,305
 $342,305^2 = 11717....$

Unfortunately the heat produced by the revolving fans was not found to be exactly proportional to the number of revolutions, or rather squared revolutions, although the figures approximated to each other much nearer on the same day than on different days. It was therefore decided to make two* preliminary experiments with the agitators on the same day as each calorimeter experiment, and to subtract the agitator calories from the total calories obtained. As the number of revolutions was not exactly the same in the test experiments and in calorimeter experiments, the calories for correction were calculated in proportion to the revolutions in the test experiment, of course previously squaring the revolutions.

The following table shows in calories the heat produced by the ventilators during forty minutes from a few experiments, the number of revolutions is about 230,000, and the calories are calculated for 200,000 :—

On 200,000 Revolutions.

Date.	Calories.	Means.
4th January, 1897.....	6866 5974	} 6420
8th „	6132 5559	
9th „	5511 5735	} 5623
10th „	5323 5780	
11th „	4901 4658	} 4779
14th „	6120 5956 6463	

* When time was pressing only one was made.

From these experiments the mean error is 218 calories, or on, say, 70,000 (emitted in forty minutes) = 0·3 per cent.

I now felt able to rely on the work undertaken with the calorimeter; still the instrument had to be tested, and with this object it was applied to the determination of the heat lost by a jar holding 6 or 7 litres of hot water and comparing the heat recovered with the heat lost by the water; the calorimeter was also used towards the estimation of the heat produced by the combustion of a known volume of pure hydrogen gas, comparing the heat recovered with that known to be produced by the combination of that volume of gas with oxygen, and this work I undertook in conjunction with R. B. Floris, F.C.S.

The first set of experiments with hot water proved very troublesome. It was found necessary to mix the layers of water in the jar before and after the experiment, and to read correctly and quickly a thermometer registering up to 0·02 of a degree centigrade; moreover the loss of heat could not be determined while the jar was being carried to and shut up in the calorimeter; and a similar difficulty was experienced on removing the jar from the calorimeter at the end of an experiment. Notwithstanding these many causes of error, as will be seen in the following table, the mean of the results approximated very closely to the calories calculated from the loss of heat of the water.

If, for instance, the jar contained 6 litres of water or 6000 grams, and lost 10° of temperature in the calorimeter, say, from 75° to 65° C., then 60,000 calories, with slight corrections for the specific heat of water at that temperature, and the thermal capacity of the jar, would have to be found in the calorimeter.

These experiments are tabulated as follows:—

Calories found.	Calories lost by radiation.	Difference.
57,451	58,468	—1·74 per cent.
49,345	53,659	—8·04 "
61,760	62,480	+1·15 "
59,432	59,141	+0·49 "
60,383	61,085	—1·15 "
63,323	64,392	—1·66 "
63,226	60,410	+4·66 "
65,882	66,575	—1·04 "
61,873	59,566	+3·87 "
63,016	66,250	—4·88 "
51,940	55,033	—5·62 "
<hr/> Mean . . 59,785	<hr/> 60,642	<hr/> —1·41 "

Although there was in one case as much as 8·04 per cent. difference between the calories found and the calories lost from the jar, still the mean of the eleven experiments differs only by 1·41 per cent., which is a near result considering the difficulty of the experiment.

The determination of the heat produced by the combustion of hydrogen was certainly a more satisfactory method than the former for testing the calorimeter; hydrogen was prepared for the purpose in the usual way by the action of sulphuric acid on zinc, the gas being purified through solutions of potassium hydrate and cupric sulphate, and collected over water in a bell-jar carefully graduated. The receiver was supplied with a gauge, showing the pressure to which the gas was subjected, and a thermometer; from 20 to 29 litres of gas were used in each experiment.

After making the required preliminary essays with the agitators, the experiment was proceeded with as follows:—

First of all it was necessary to find out and to adjust carefully the speed of the gas delivery, and with that object a weight was placed on the bell-jar, while the rate of issue of the gas was regulated at will by means of a screw clamp on the track of the gas tube.* In that way the speed of the gas delivered was adjusted so as to produce on burning about the same heat as a person would emit in the calorimeter in a given time.

The delivery tube led from the bell-jar into the calorimeter through a fixed metal tube carried across the walls of the two chambers of the calorimeter, its end being connected with a suitable burner; when lighted, the gas burnt with but a very small flame.

Before commencing the experiment the tube was rinsed out with hydrogen and the thermometers were read, together with the pointer on the scale of the bell-jar. Then the gas was turned on and lighted, the vessel containing the ice hung in position, the stop-watch started, the calorimeter closed, and the agitators put in motion. Of course every care was taken to keep the temperature of the calorimeter constant, which was done without any difficulty, the temperature of the copper varying seldom by more than 0·1° or 0·2° C.

When forty minutes or an hour had elapsed (mostly forty minutes), the temperatures were read, the gas turned off, and the agitators stopped. Next the calorimeter was rapidly opened, and the flow of water from the ice to the flask arrested; the temperature of the ice water was then read off, these various operations being carried out as rapidly as possible. It was necessary to determine the heat absorbed by the burner, which was done by plunging the burner, immediately after turning off the gas, into 200 c.c. of water at a

* The gas was carried as much as possible through glass tubing, in order to avoid the loss by diffusion through india-rubber.

known temperature and determining the rise of temperature of the water; the burner was found to absorb 300 calories during the experiment. The pointer showed on the scale of the bell-jar the volume of hydrogen burnt, and the gauge the pressure the gas was under in millimetres of water, while a thermometer gave the temperature of the gas in the bell-jar, and a barometer the atmospheric pressure; the hydrogen gas was of course saturated with water vapour. Hence we were in possession of every data for the reduction of the gas to the dry state, to 0° C., and 760 mm. pressure.

In the early experiments it did not occur to us to analyse the hydrogen gas in order to ascertain its degree of purity, but we did so subsequently, using for that purpose the eudiometer constructed by one of us (W. M.), which for several years has been exclusively adopted in this laboratory for the determination of oxygen in expired air; the analysis of the gas introduced but a very slight correction. The following table gives the results of the experiments we made on the heat emitted by the combustion of a given weight of hydrogen gas. It might be added that the machine known as "Brunsviga" was used for the calculations, which saved much time and trouble; by this means the whole of the calculations could be completed in about fifteen minutes.

Favre and Silbermann find 1 gram of H to give in burning 34,462 calories.

	Found.	Difference.
1	33,334	3.26 low per cent.
2*.....	33,159	3.78 " "
3*.....	35,291	2.41 high "
4	35,186	2.10 " "
5*.....	34,212	0.73 low "
6*.....	35,610	3.33 high "
7	33,923	1.56 low "
8	34,079	1.11 " "
9	34,440	0.06 " "
10	35,048	1.70 high
Mean....	34,428	0.10 low "
Favre and Silbermann.....	34,462	
Marcet and Floris.....	34,428	

$$34 = 0.1 \text{ per cent.}$$

The present result is certainly convincing, and these figures are plain statements of all the experiments we made. The greatest

* In these experiments the hydrogen gas was not analysed; it was analysed in all the others and the correction therefrom introduced.

difference is only one of 3·78 per cent., and the mean difference did not exceed 34 calories on 34,462, amounting to 0·1 per cent. only. It may therefore be concluded that the present calorimeter has proved itself very accurate for the determination of the heat produced by the combustion of a given volume of hydrogen gas; and, consequently it can be accepted as equally reliable for the correct estimation of the heat radiated from the human body or from that of a fairly large animal.

“An Experimental Enquiry into the Heat given out by the Human Body.” By W. MARCET, M.D., F.R.S., and R. B. FLORIS, F.C.S. Received March 10,—Read April 28, 1898.

(From the Physiological Laboratory of University College, London.)

Dr. Marcet's calorimeter having been fully described in the previous paper, the present conjoint authors now submitted themselves to experiment, one of them remaining shut up in the chamber, usually for the space of an hour, while the other was engaged outside to regulate the temperature of the chamber and note the readings of the thermometers.

When breathing was carried on inside the calorimeter, it might be thought that the air of the chamber became too full of CO₂ or too deficient in oxygen for the purposes of respiration. Such, however, was not the case, and no discomfort whatever was experienced in the course of an hour's incarceration. It is easy to calculate from a consumption of, say, 26·488 grams of O per hour that supposing the calorimeter to be absolutely air-tight, a condition which was not actually realised, there would be a fall of oxygen, after one hour spent in the calorimeter, equal to a reduction of pressure from 760 mm. to 668 mm., and this would correspond to an elevation of about 7000 feet (2135 metres) above the sea level. Such an altitude would certainly not be trying to the respiration.

The experiment was carried out as follows in every instance:—

Previous to entering the chamber the subject of the experiment sat down in the laboratory to rest, in many instances taking his temperature, sublingual, with a clinical thermometer.

In the meantime a weight of ice varying between 10 lbs. and 15 lbs. (4·5 to 6·8 kilograms), according to circumstances, was cut into blocks about 2 or 3 inches diameter, and placed in the ice holder, where the blocks were disposed as much as possible in a position to allow the air from the agitator to circulate freely between them. A temporary receiver for the water from the melting ice was hung to a

hook soldered to the tube delivering the ice water, for which receiver a flask of a capacity of over a litre was substituted on starting the experiment in the chamber.

When all was ready, the person under experiment stepped into the calorimeter and sat down on a wooden chair. Immediately afterwards the ice holder was hung up in the chamber to a strong hook fastened to the roof; then the tube from the upper agitator was connected with the ice holder, and the flask with a thermometer in it, previously weighed, was substituted for the temporary small receiver of the melted ice. At that very instant the stop-watch was started to register the time spent in the chamber, the door was closed and screwed down, and the two agitators were set in motion. It might here be observed that the person in the calorimeter felt no draught, as the air from the lower agitator was driven up behind him through a rose-jet, and that from the upper agitator fell in front of him on its exit from the ice holder. Indeed no sensation of cold was experienced, or any discomfort whatever, the temperature in the chamber remaining exactly the same within a few tenths of a degree centigrade throughout the experiment. In those cases where the air expired had to be collected for analysis, this was done by means of a face-piece strapped to a cap fitting the back of the head. The face-piece was supplied with a glass tube, which was taken between the lips and used for the expiration, while another tube served for the nasal inspiration from the outside of the chamber, without the intervention of any valves. Fresh air was thus inspired through the nose and expired through the mouth—a method of breathing with which we were familiar. The inspiratory tube communicated with the external air through the walls of the chambers, while the expiratory tube was connected at will either with the bell-jars or the open air. The bell-jars were suspended in such a way that the person in the chamber never knew when he was breathing into the open air or into the air holders. Three bell-jars were in use, and in many cases an india-rubber bag, faced with oil-silk, was pressed into service, so as to allow of the collection of an increased volume of expired air. The volume of air collected, though only including the air expired during from twenty minutes to half an hour, taken at intervals through the whole time, certainly gives an accurate estimation of the composition of all the air breathed while in the calorimeter, considering that the person remains in a perfect state of repose during that time, except, perhaps, for a minute on entering the chamber.

On one occasion the whole of the air expired in one hour was collected, and in that experiment it was found that the volume of air expired in half an hour, taken at different intervals of time, was proportional to the volume expired in the whole hour.

The observer, whose duty it was to read the thermometers, stood up on a stool ready to work the regulators of the dynamos, and by constant attention the temperature of the metal of the copper chamber was not allowed to fall or rise beyond about 0.3° from the initial reading.

After an hour, measured to a second with the stop-watch, a last reading of the thermometers was taken while at the same time a signal was made to the person in the chamber by showing him a light, and he immediately closed the stopcock of the tube letting the melted water into the flask. By this means the water collected was given out exactly in one hour, the agitators were stopped, and the person under experiment finally let out. As soon as the door was open, the temperature of the ice water in the flask was recorded.

The sublingual temperature when required was again taken at that time, if it had not been determined in the last few minutes of the stay in the chamber.

The next process was to weigh the flask with the ice water and thermometer, and by subtracting the weight of the empty flask and thermometer, that of the melted ice was obtained. In one hour's experiment the water from the melted ice amounted to rather over 1000 grams, but its weight varied on each occasion. The counters in connection with the agitators were now read.

This completed the data for the calculation of the calories recovered in the chamber. The calculation, a very simple one, is illustrated in the following table, which gives the particulars of one experiment taken at random:—

Illustration of the Observations and Calculations required in one Experiment.

Two preliminary Test Experiments with Agitators to determine Heat given out.

		Upper agitator.	Lower agitator.
1st counters.	{ Final readings ..	924,442	677,155
	{ Initial „ ..	735,640	494,300
	Number of revolutions ..	188,804	182,855
2nd counters	{ Final readings ..	1,104,775	852,831
	{ Initial „ ..	924,444	677,155
	Number of revolutions ..	180,331	175,676
	Sum of revolutions squared.	Calories found.	
1st	13,813	12,527	
2nd	12,674	12,138	
Means	13,244	12,333	

Experiment with Subject in Calorimeter.

Counters	{	Final readings....	Upper agitator.	Lower agitator.
			Initial „	Initial „
			238,566	1,052,431
			104,775	852,831
			<hr/>	<hr/>
			133,791	199,600

$$\begin{aligned} & 133,791 \\ & \cdot 199,600 \\ & \hline & (333,391)^2 = 11,115.... \end{aligned}$$

$$\begin{aligned} 13,244 : 11,115 &= 12,333 : x \\ x &= 10,355 \end{aligned}$$

	Chamber.	Copper.	Annular space.
3.51 P.M.	15.30° C.	15.16° C.	15.20° C.
4.1	15.33	15.26	15.26
4.11	15.34	15.13	15.32
4.21	15.62	15.22	15.32
4.31	15.28	15.12	15.32
4.41	15.74	15.22	15.34
4.51	15.12	15.10	15.35
	<hr/>	<hr/>	<hr/>
Difference of extremes	-0.18	-0.06	+0.15

Time spent in calorimeter, one hour.
Weight of ice water, 1309.30 grams.
Temperature of water, 9° C.

Calories found.

	Calories.
Ice water	1309.30 × 79* = 103,435
Ice water	1309.30 × 9° = 11,784
From air in annular space.....	69 × 0.15 = 10
From copper†	5832 × 0.06 = 350
From air in chamber ..	214 × 0.18 = 38
	<hr/>
	388
	<hr/>
Calories	114,841
Correction for agitators	10,355
	<hr/>
Heat emitted in one hour, say, calories found	104,486

* 79 = number of calories absorbed by the melting of 1 gram of ice.

† The figures 5832 for copper, 214 for air of chamber, and 69 for air of annular space are constants obtained by multiplying the individual weights by their corresponding specific heat.

Determination of Oxygen consumed or absorbed.

130.427 litres of air expired (reduced) in 26 minutes.

	Per minute.
CO ₂ found	239.0 c.c.
Surplus O absorbed	65.1

 304.1×1.4338 (weight of 1 litre of O)
 $\times 60$ minutes = 26.162 grams O absorbed per hour.

104,486/26,162 = 3994 calories, corresponding to 1 gram O consumed.

The first set of experiments was undertaken in order to ascertain whether in the course of an hour there is any variation in the heat given out by the same person, and, with this object in view, the heat emitted by each of us in turn was determined throughout the first and second half-hours spent in the calorimeter. It was found necessary to introduce a three-way cock into the tube delivering the water from the melted ice into the flask; this three-way cock diverted the stream at will into one or other of two flasks hanging to the tubes leading from the ice-holder.

The results obtained were as follow :—

Calories in two half-hours in succession.

W. M.		R. B. F.	
First half-hour.	Second half-hour.	First half-hour.	Second half-hour.
58,781	58,551	55,444	51,395
53,230	50,939	49,216	52,445
55,327	52,161	49,128	53,812
59,947	62,177	48,037	48,914
46,644	48,954	49,540	51,905
—	—	46,744	44,878
Means 54,786	54,556	49,685	50,558

Difference = 0.42 per cent.
decrease in second half-hour on first half-hour.

Difference = 1.76 per cent.
increase in second half-hour on first half-hour.

Total for the eleven experiments, 0.75 per cent. increase in second half-hour. (Calculation from total figures.)

Therefore, in the case of W. M., the mean of five experiments gave a difference of only 0.42 per cent. in the calories emitted during two successive half-hours, while, in the case of R. B. F., the mean difference of

six experiments was only by 1.76 per cent. The mean difference (from total figures) from the two persons in eleven experiments amounted to 0.75 per cent., showing that practically the mean heat emitted was the same in each of two consecutive half-hours. There were, however, differences, though usually slight, in each pair of experiments—sometimes an increase, sometimes a decrease—the reason of which is difficult to assign.

The calories given out by the various persons experimented upon were taken generally between lunch and dinner, say at a mean time of about two hours after a full luncheon, and therefore under the immediate influence of food. But towards the end of the inquiry a certain number of experiments were made just before lunch, corresponding with others made after lunch, in order to determine in a general way the effect of a full mid-day meal on the heat-producing power of the body. The mean of seventy-two experiments on four persons, aged respectively 15, 27, 28, and 69, gave 102,907 calories per hour,* and varied from 80,639 to 137,078. In other words, the mean heat given out in one hour was such as would raise 102,907 grams of water by 1° centigrade (from 0° to 1°).

The next point we submitted to enquiry was the relation, if any, between the oxygen absorbed from the air breathed, and the calories emitted at the same time.

The oxygen absorbed was determined by collecting the air expired by the person in the calorimeter, and estimating the CO₂ and O contained in the expired air. This was done by methods fully described in previous papers (by W. M.), and need not be further insisted upon. We found the method of breathing for collecting the air expired (inspiration through nose and expiration through mouth) quite satisfactory in every way, the subjects for these experiments being all used to this mode of breathing.

It is important to observe at the outset that, while there were great differences between the calories found for each person, the oxygen absorbed from the air in every individual case did not exhibit such marked variations; moreover, except in a very general way, the oxygen absorbed failed to vary in proportion with the number of calories emitted.

* These experiments include the whole number made, most of them under the influence of a full meal, but a few fasting, or before the mid-day meal. Of course they can only be expected to give a general idea of the mean calories emitted by man, as the amount of heat emitted varies with every different person, and under different conditions as to food and many other circumstances.

Calories emitted in one hour and corresponding Oxygen absorbed.
W. M. under experiment. Age 69; weight 57.9 kilograms.

Calories in one hour.	Change of sublingual temperature.	Time for collecting air expired.	O absorbed.	Calories for 1 gram O absorbed.
	° C.	mins. secs.		
122,124	—	8 2	24.276	5081
110,654	—	7 48	25.592	4324
94,837	—	7 30	22.581	4200
98,905	0.0	21 0	24.862	3855
98,408	—	15 24	21.697	4305
92,781	-0.25	32 28	21.721	4269
*91,270	-0.2	54 35	24.233	3766
*90,882	—	21 36	22.615	4019
90,844	+0.1	21 50	20.611	4408
*90,155	-0.1	24 3	21.764	4143
*89,085	-0.2	20 3	25.015	3561
89,011	+0.05	15 45	21.643	4113
88,832	—	24 0	21.961	4045
88,406	0.0	27 43	21.867	4043
*87,240	-0.3	28 0	22.830	3821
81,315	-0.15	One hour	23.062	3526
80,639	-0.1	29 29	24.495	3292
				Mean 4042
Supplementary List.				
107,397	—			
104,169	—			
101,151	+0.15			
98,123	-0.1			
97,974	—			
96,399	-0.25			
95,598	—			
88,641	—			
87,314	-0.2			

* The figures for calories with an asterisk correspond to experiments made in the calorimeter with an overcoat on. The effect of the increased clothing is insufficient to influence the general means.

Calories emitted in one hour and corresponding Oxygen absorbed.
R. B. F. under experiment. Age 27 ; weight 53·0 kilograms.

Calories in one hour.	Change of sublingual temperature.	Time for collecting air expired.	O absorbed.	Calories for 1 gram O absorbed.
	° C.	mins. secs.		
105,203	0·0	26 0	26·458	3976
103,569	—	8 50	23·249	4455
103,186	-0·05	23 0	23·352	4419
101,445	—	9 10	23·266	4360
100,077	0·0	21 0	26·785	3736
97,071	—	9 20	22·131	4386
96,947	—	18 0	22·286	4350
95,924	-0·3	22 0	23·215	4132
92,880	-0·05	23 17	25·443	3651
91,622	—	7 40	26·312	3482
91,558	0·05	20 0	25·951	3528
81,882	-0·3	28 0	23·533	4379
80,985	-0·45	15 41	23·593	3433
				Mean 4022
Supplementary List.				
106,839				
102,940				
101,661				
100,159	-0·25			
99,309	-0·05			
96,735	-0·2			
90,523	-0·25			
85,338	-0·4			

250 Dr. W. Marcet and Mr. R. B. Floris. *An Experimental*

Calories emitted in one hour and corresponding Oxygen absorbed.
E. R. under experiment. Age 28; weight 80·4 kilograms.

Calories in one hour.	Change of sublingual temperature.	Time for collecting air expired.	O absorbed.	Calories for 1 gram O absorbed.
	° C.	mins. secs.		
126,928	+0·1	26 0	30·762	4126
124,335	—	13 45	30·665	4055
117,181	+0·05	28 32	29·290	3990
116,874	-0·3	12 2	30·237	3865
115,221	-0·15	24 41	28·379	4060
111,754	0·0	22 2	32·887	3396
				Mean 3916
Supplementary List.				
137,078	-0·3			
129,028	0·0			
127,594	+0·15			
126,872	+0·1			
125,129	—			

These lists include thirty-six experiments on three different persons, in all of which the oxygen taken from the air, while in the chamber, was determined. At the outset a striking similarity is observed between the *means* of the calories produced for 1 gram of oxygen absorbed.

(Mean of 17 exp.)	With W. M. that figure is...	4042
(" 13 ")	" R. B. F. "	4022
(" 6 ")	" E. R. "	3916

It is, therefore, obvious that there is a definite relation of cause to effect in the absorption of oxygen towards the production of animal heat. These figures must not be considered as absolutely final; they show that under similar circumstances relating to time of food, &c., the mean calories produced for 1 gram of oxygen absorbed are the same for different persons, at all events as far as can be gathered from the three subjects experimented upon; and, moreover, it may be concluded that the true figure closely approximates 4000 (small calories). Hirn, from his experiments, gives 5·22 large calories for 1 gram of oxygen absorbed, which exceeds our figure (4·00) to a marked extent.

If the volume of oxygen absorbed from the air was proportional to the calories given out during the same time, then, by placing in a *tabular form* the numbers for the calories found, beginning with the

highest number and ending with the lowest, and also inscribing opposite these figures those showing the oxygen absorbed in each corresponding experiment, it would be expected that the figures for oxygen absorbed would follow in succession those for the calories found, beginning with the highest and ending with the lowest. Such, however, is far from being the case. Therefore, except in a very general way, as shown by our *means* and under similar circumstances with reference to food, the oxygen taken from the air *does not produce heat in the body in proportion to the amount absorbed*. And this may be taken as a clear indication that the oxygen absorbed in a given time is not a measure of the heat produced during that same time.

Should tables now be made of the calories in numerical order, beginning with the highest, placing the figure for "calories for 1 gram oxygen absorbed" opposite its respective calories, a peculiar occurrence is observed. It will be seen readily by a consideration of the foregoing tables that if the calories found in one hour and those calculated for 1 gram oxygen absorbed be grouped three by three (or even two by two), the calories for 1 gram oxygen absorbed decrease fairly regularly together with the falling for the hour-calories (less so when taken two by two), and this takes place in the case of three different persons, and therefore cannot possibly be accidental. It follows that 1 gram of oxygen absorbed from the air is attended with the emission of either 5031 calories or 3292 calories, as extremes for one person, and for another person either 4455 or 3433 calories, and again for a third either 4126 or 3398 calories. The readiest explanation of this phenomenon is the assumption of a storage of oxygen in the tissues, which is made use of, although unaccounted for (as oxygen absorbed) at the time. Still, the *mean* relation of the oxygen absorbed to the heat emitted remains the same, being as nearly as possible 4000 calories for 1 gram oxygen absorbed under similar conditions.

This shows that whatever be the mode of action of the absorbed oxygen it repeats itself in a general way, if taken at similar periods with reference to food, &c.

There is a circumstance in these experiments which should be taken into account concerning the frequent change of temperature of our bodies while in the calorimeter, as ascertained by sublingual observations with a clinical thermometer. The cooling reached an extreme of 0.45° C., though usually only 0.15° or 0.2° C., but it varied much in different experiments, while on some occasions there was no change, and even once or twice a rise was observed. The fall of temperature was thought at first to be due to the proximity of the ice in the chamber; but the same effect was observed by taking W. M.'s sublingual temperature while sitting quite quiet for an hour

out of the calorimeter, when a distinct and gradual fall in the sublingual temperature occurred to the extent of 0.2°C .

We looked carefully through the results obtained, in order to ascertain whether this cooling of the body while under experiment had any appreciable influence either on the heat emitted, or on the oxygen absorbed, but failed to observe any phenomena which might be ascribed to such a cause; it must, however, be attended with some effect.

A few experiments were made on the influence of food in a general way by comparing the heat given out in the calorimeter by the same individual, shortly before, and one or two hours after the midday meal, the meal consisting of a full allowance of meat, potato, bread, and a glass of beer. The experiments before luncheon are, therefore, in a comparatively fasting condition, and those after luncheon may be looked upon as being under the influence of a full meal.

W. M.		
Before lunch.	After lunch.	Per cent. increase after lunch.
92,731	96,399	3.95
88,406	98,123	10.99
(90,844	87,314	fall)
81,315	101,151	24.39

R. B. F.		
95,924	99,309	3.53
81,882	84,653	3.38
80,985	91,208	12.62
96,050	99,474	3.56

E. R.		
111,754	125,129	11.97
115,221	126,872	10.11
116,874	129,028	10.40
117,131	127,594	8.93
126,928	135,870	7.04

E. F.		
(118,087	114,377	fall)
121,868	125,001	2.57
(114,675	110,495	fall)
98,643	109,730	11.24
104,922	108,952	3.84
109,078	112,780	3.39
<hr/> 667,273	<hr/> 681,335	

Therefore out of nineteen experiments only three show a fall in the amount of heat emitted after lunch. Of these three, one (W. M.) is easily accounted for from the digestion of the person under experiment being on that day somewhat out of order, and very little food being taken. In the other instances, in which the laboratory boy was in the calorimeter, he acknowledged finding it difficult to sit quiet, and movement may easily account for the irregular result.

It follows from these experiments that the rule is an increase in the emission of heat from the body after a full meal.

Finally it was of interest to ascertain how far the heat emitted by the body is in proportion to the weight of the body. The following table shows clearly that this relation is subject to great variations; the lightest person under experiment, also the youngest (being sixteen years of age), gave out a mean amount of heat per kilo. weight greatly exceeding all the others.

	Calories per hour.	Weight of the body.	Mean calories per kilogram.
W. M. (mean of 26 experiments)...	95,605	57·9	1651
R. B. F. " 21 " ...	96,469	53·0	1820
E. R. " 11 " ...	123,449	80·4	1535
E. F. " 12 " ...	112,217	41·0	2737

When the work connected with this paper was nearly completed, it occurred to us that there were two important omissions in the enquiry. The first was the neglect to take into account the heat lost from the calorimeter by the air expired when it was collected for analysis; * on the other occasions the air was expired into the calorimeter, and therefore there was no loss of heat from that cause. We now made a few experiments, taking the temperature of the air expired at its exit from the chamber. The excess of this temperature over that of the air inspired was used for calculating the heat lost, and the correction introduced where necessary. This figure varied somewhat for each person under experiment. The second omission was leaving the carbonic acid which might have collected

* Supposing the air expired was found on its exit from the calorimeter to be 8° C. higher than the external air, and that this volume of air reduced amounted to 112·5 litres in 25 minutes, this would give a weight of 145·5 grams of air, which multiplied by the specific heat of air (0·2375) and by 8, the excess temperature = 276·4 calories, or for an hour 663 calories. On a total heat of 95,000 calories, the heat thus lost would only amount to 0·7 per cent.

in the calorimeter unaccounted for. Some CO_2 must be derived from the skin, and perhaps some small quantity of air might possibly have escaped from the face-piece into the chamber.

The calorimeter was conveniently available for the determination of the CO_2 , which might have collected in it, as the chamber was perfectly closed, whatever minute openings there were being much too small to allow of any diffusion out of the chamber. This was tested experimentally in the course of the present enquiry by expiring air into the bell-jars, while out of the calorimeter; shortly afterwards the same person entered the chamber and breathed in it for forty minutes or an hour. The mean of eight experiments gave figures for the CO_2 in and out of the chamber exactly the same.*

The determination of the CO_2 in the air of the chamber while breathing the external air was made by putting on the face-piece on entering the calorimeter, and after the door was closed, rinsing a dry flask (holding about 6 litres) with air from the upper agitator; this flask was then stopped with an india-rubber cork, having a tube with a stopcock inserted through it. After sixty minutes another large, dry flask, full of fine, dry sand, was suddenly emptied of its contents, and closed with a cork similar to that used with the other flask. The determination of CO_2 was made in each flask with a standard solution of barium hydrate, by Pettenkofer's method. The CO_2 in the chamber was obtained by subtracting the weight (or volume) of CO_2 found in the chamber at the very beginning of the experiment from the weight of the CO_2 found in the chamber after the subject had remained an hour in it. This CO_2 varied somewhat with the three persons who submitted to experiment; the mean value for each of us was used for the correction of the oxygen absorbed; this correction was, however, but small, varying from about 1 to 3 per cent. of the CO_2 determined in the air collected in the bell-jar.

The results obtained from the present enquiry may be summarised as follows:—

1. The amount of heat given out from the human body when

*	CO_2 in bell-jars per minute.	Time for collecting in bell-jar.		CO_2 in chamber per minute.
	c.c.	Mins.	Secs.	c.c.
	204.4	9	20	199.2
	181.5	8	3	176.1
	176.0	9	16	183.5
	208.6	7	41	214.6
	200.1	8	25	194.7
	187.9	8	31	200.2
	205.0	3	50	194.9
	<hr/> 194.8			<hr/> 194.7

tested on two successive half-hours is found to be the same when the means of the calories are taken, although in each separate experiment the heat emitted may vary to some slight extent.

2. The heat emitted by the same person varies, and the extent of this variation is wider in some subjects than in others; thus in W. M.'s case the calories emitted in one hour from twenty-six experiments varied from 122,124 to 80,639, or by 33·9 per cent. of the larger figure. In the twenty-one experiments of R. B. F. they varied from 106,839 to 80,985, or by 24·2 per cent. With E. R., in eleven experiments, from 137,078 to 111,754, or by 18·5 per cent.

3. As a fact irrespective of theory the mean number of calories found from three different persons, under similar circumstances of food, &c., corresponding to 1 gram of oxygen absorbed from the air, was the same, and can be stated in round numbers at 4000. Had more experiments been done fasting, this figure would have shown a slight tendency to fall.

4. Although the *mean* calories per individual for 1 gram oxygen absorbed under similar circumstances of food, &c., are the same, still in the experiments taken singly, the number of calories corresponding to 1 gram oxygen absorbed, vary, and this in a regular way. The greater the heat given out, the greater the calories produced for 1 gram oxygen absorbed, and *vice versâ*. Therefore, either a given amount of oxygen absorbed can produce different quantities of heat, or the oxygen found as absorbed does not represent that to which the heat is due; this second alternative appears the more probable.

5. The influence of a meal, as ascertained on three different persons, is well marked. Taking the midday meal, when mixed food is eaten, generally with a good appetite, the calories emitted about two hours after lunch show an increase over those given out about three or four hours after breakfast. The excess varies in different persons, and according to the kind and amount of food taken.

6. The calories emitted per kilo. weight of the body are subject to marked variations in different persons.

A few experiments were made on the influence of clothing on the heat emitted, but we thought it best to reserve that subject for a future communication.

"Preliminary Note on the Liquefaction of Hydrogen and Helium." By JAMES DEWAR, M.A., LL.D., F.R.S., Full-
erian Professor of Chemistry in the Royal Institution.
Received and read May 12, 1898.

In a paper entitled "The Liquefaction of Air and Research at Low Temperatures," read before the Chemical Society, and published in their 'Proceedings,' No. 158, an account is given of the history of the hydrogen problem and the result of my own experiments up to the end of the year 1895. The subject is again discussed in a Friday Evening Lecture on "New Researches on Liquid Air,"* which contains a drawing of the apparatus employed for the production of a jet of hydrogen containing liquid. It was shown that such a jet could be used to cool bodies below the temperature that could be reached by the use of liquid air, but all attempts to collect the liquid in vacuum vessels failed. No other investigator has so far improved on the results described in 1895. The type of apparatus used in these experiments worked well, so it was resolved to construct a much larger liquid air plant, and to combine with it circuits and arrangements for the liquefaction of hydrogen, which will be described in a subsequent paper. This apparatus, admirably constructed by the engineers, Messrs. Lennox, Reynolds, and Fyfe, took a year to build up, and many months have been occupied in testing and making preliminary trials. The many failures and defeats need not be detailed.

On May 10, starting with hydrogen cooled to -205°C. , and under a pressure of 180 atmospheres, escaping continuously from the nozzle of a coil of pipe at the rate of about 10 cubic feet to 15 cubic feet per minute, in a vacuum vessel double silvered and of special construction, all surrounded with a space kept below -200°C. , liquid hydrogen commenced to drop from this vacuum vessel into another doubly isolated by being surrounded with a third vacuum vessel. In about five minutes, 20 c.c. of liquid hydrogen were collected, when the hydrogen jet froze up from the solidification of air in the pipes. The yield of liquid was about 1 per cent. of the gas. The hydrogen in the liquid condition is clear and colourless, showing no absorption spectrum and the meniscus is as well defined as in the case of liquid air. The liquid has a relatively high refractive index and dispersion, and the density appears to be in excess of the theoretical density, viz., 0.18 to 0.12, which we deduce respectively from the atomic volume of organic compounds and the limiting density found by Amagat for hydrogen gas under infinite

* 'Roy. Inst. Proc.,' 1896.

compression. My old experiments on the density of hydrogen in palladium gave a value for the combined body of 0.62, and it will be interesting to find the real density of the liquid substance at its boiling point. Not having arrangements at hand to determine the boiling point, two experiments were made to prove the excessively low temperature of the boiling fluid. In the first place, if a long piece of glass tubing, sealed at one end and open to the air at the other, is cooled by immersing the closed end in the liquid hydrogen, the tube immediately fills, where it is cooled, with solid air. The second experiment was made with a tube containing helium.

The 'Cracow Academy Bulletin' for 1896 contains a paper by Professor Olszewski, entitled "A Research on the Liquefaction of Helium," in which he states "as far as my experiments go, helium remains a permanent gas and apparently is much more difficult to liquefy than hydrogen." In a paper of my own in the 'Proceedings of the Chemical Society,' No. 183 (1896-7), in which the separation of helium from Bath gas was effected by a liquefaction method, the suggestion was made that the volatility of hydrogen and helium would probably be found close together just like those of fluorine and oxygen. Having a specimen of helium which had been extracted from Bath gas, sealed up in a bulb with a narrow tube attached, the latter was placed in liquid hydrogen, when a distinct liquid was seen to condense. A similar experiment made with the use of liquid air under exhaustion in the same helium tube (instead of liquid hydrogen) gave no visible condensation. From this result it would appear that there cannot be any great difference in the boiling points of helium and hydrogen.

All known gases have now been condensed into liquids which can be manipulated at their boiling points under atmospheric pressure in suitably arranged vacuum vessels. With hydrogen as a cooling agent, we shall get within 20° or 30° of the zero of absolute temperature, and its use will open up an entirely new field of scientific inquiry. Even as great a man as James Clerk Maxwell had doubts as to the possibility of ever liquefying hydrogen.* No one can predict the properties of matter near the zero of temperature. Faraday liquefied chlorine in the year 1823. Sixty years afterwards Wroblewski and Olszewski produced liquid air, and now, after a fifteen years' interval, the remaining gases, hydrogen and helium, appear as static liquids. Considering that the step from the liquefaction of air to that of hydrogen is relatively as great in the thermo-dynamic sense as that from liquid chlorine to liquid air, the fact that the former result has been achieved in one-fourth the time needed to accomplish the latter, proves the greatly accelerated rate of scientific progress in our time.

* See 'Scientific Papers,' vol. 2, p. 412.

The efficient cultivation of this field of research depends upon combination and assistance of an exceptional kind; but in the first instance money must be available, and the members of the Royal Institution deserve my especial gratitude for their handsome donations to the conduct of this research. Unfortunately its prosecution will demand a further large expenditure. The handsome contribution made by the Goldsmiths Company ought also to be mentioned as very materially helping the progress of the work.

During the whole course of the low temperature work carried out at the Royal Institution, the invaluable aid of Mr. Robert Lennox has been at my disposal; and it is not too much to say that but for his engineering skill, manipulative ability, and loyal perseverance, the present successful issue might have been indefinitely delayed. My thanks are also due to Mr. J. W. Heath for valuable assistance in the conduct of these experiments.

“Effects of Prolonged Heating on the Magnetic Properties of Iron.” By S. R. ROGET, B.A. Communicated by Professor EWING, F.R.S. Received April 4,—Read May 12, 1898.

It has been known for some years that when transformers are kept in use, their open-circuit loss is liable to increase considerably with the lapse of time. This implies a deterioration of the iron core in regard to magnetic hysteresis. The subject began to receive attention in 1894–5, when some curves showing this increase in hysteresis were published by Mr. G. W. Partridge.* The effect was first thought to be due to a species of magnetic fatigue, resulting from repeated reversals of magnetism in the iron, but it was proved by Professor Ewing early in 1895 not to be due to this cause;† and was further shown by the experiments of Mr. Bláthy and Mr. Mordey to be a direct effect of heat and to occur in transformers as a consequence of the iron being maintained for long periods at a comparatively high temperature. Continued baking of iron was found to produce a similar augmentation of hysteresis. The published results of Mr. Mordey,‡ and, later, those of Mr. Parshall,§ deal with prolonged heating at temperatures which do not exceed 140° C. At the suggestion of Professor Ewing, the author has been carrying out, in the laboratory of the Engineering Department of Cambridge University, some investigations which deal with a more extended range of temperature. The experiments are still in progress, but

* ‘The Electrician,’ vol. 34, p. 160, December, 1894.

† *Ibid.*, p. 297, January, 1895.

‡ ‘Proc. Roy. Soc.,’ vol. 57, p. 224, June, 1895.

§ ‘Min. Proc. Inst. C.E.,’ vol. 126, p. 244.

already results have been obtained which appear to be of sufficient novelty to warrant publication.

The hysteresis of the iron was directly measured by means of Professor Ewing's Hysteresis Tester, in which the work spent on a specimen rotating in a magnetic field is observed and is compared with the work spent in rotating standard specimens.* In these experiments the same pair of standards was used throughout for the calibration of the hysteresis tester. The test specimens were all cut from the same sheet of metal and were of soft Swedish transformer plate, having very low initial hysteresis. They were first tested in the annealed state and were then heated in small ovens which were kept hot by means of incandescent lamps. The temperatures of the ovens were observed in most cases by mercury thermometers, but those above 200° C. were measured by a Callendar-Griffiths platinum pyrometer. The specimens were taken out of the ovens from time to time to be tested, and all the tests of hysteresis were made at atmospheric temperature. It was not found possible to keep the temperature of each oven very constant, but when the ovens were once hot, the variation of temperature was rarely more than 10 degrees C. in either direction. To these variations may be ascribed certain irregularities which will be apparent in the observations, but the general character of the changes due to prolonged heating is sufficiently clear. Each specimen consisted of a bundle of seven strips 3 inches long, and about $\frac{5}{8}$ inch wide, and each strip was annealed separately by heating it to redness in a Bunsen flame, and allowing it to cool in the air. As the effects of prolonged heating described below were in all cases found to be completely removed by reannealing, the same samples could be used over and over again, and this was in fact done in most cases. In all the experiments the measurement of hysteresis relates to cyclic processes in which the induction B changes from + 4000 to - 4000 C.G.S. units.

The effects produced by baking differ widely at different temperatures. Below 40° C. the author has found no evidence of any change. Between 40° C. and about 135° C. the hysteresis simply increases with time, at least during the longest time of heating tried in these experiments. The increase of hysteresis is relatively rapid at first, and becomes slower as time goes on. Curves 1-4, fig. 1 show results of this nature by exhibiting the percentage increase in hysteresis after various times of baking. The absolute values of the hysteresis at the different stages are stated in Table I in ergs per cycle per cubic centimetre (for $B = 4000$) together with the rise expressed as a percentage of the initial hysteresis to the nearest

* 'Journal Inst. Elect. Eng.,' vol. 24, p. 403; also 'Min. Proc. Inst. C.E.,' vol. 126, p. 206.

1 per cent. The curves have been sketched by joining the observed points instead of drawing smooth curves through them, as this avoids confusion of points belonging to different curves.

It was found however that at higher temperatures, from about 135°C. upwards, a maximum value of the hysteresis was attained in a comparatively short time, after which continued heating caused a marked *decrease* of hysteresis instead of a further increase. The initial rise at the higher temperatures is very rapid; for example, the hysteresis doubles in a few hours at a temperature of 160°C. , and reaches nearly three times its initial value in a few days. Curve 5 of fig. 1 exhibits this case, the data for which are given in Table I. After seven days of heating, the hysteresis of this sample began to decrease, and in fifteen days it had fallen to $2\frac{1}{2}$ times its original value. A still more notable decrease occurs at higher

FIG. 1.

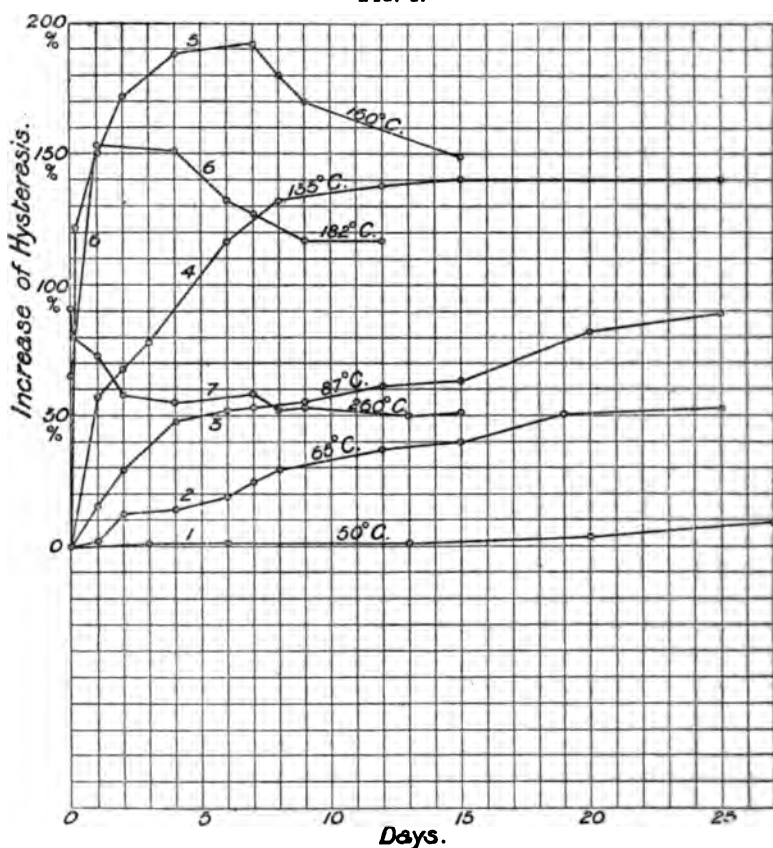
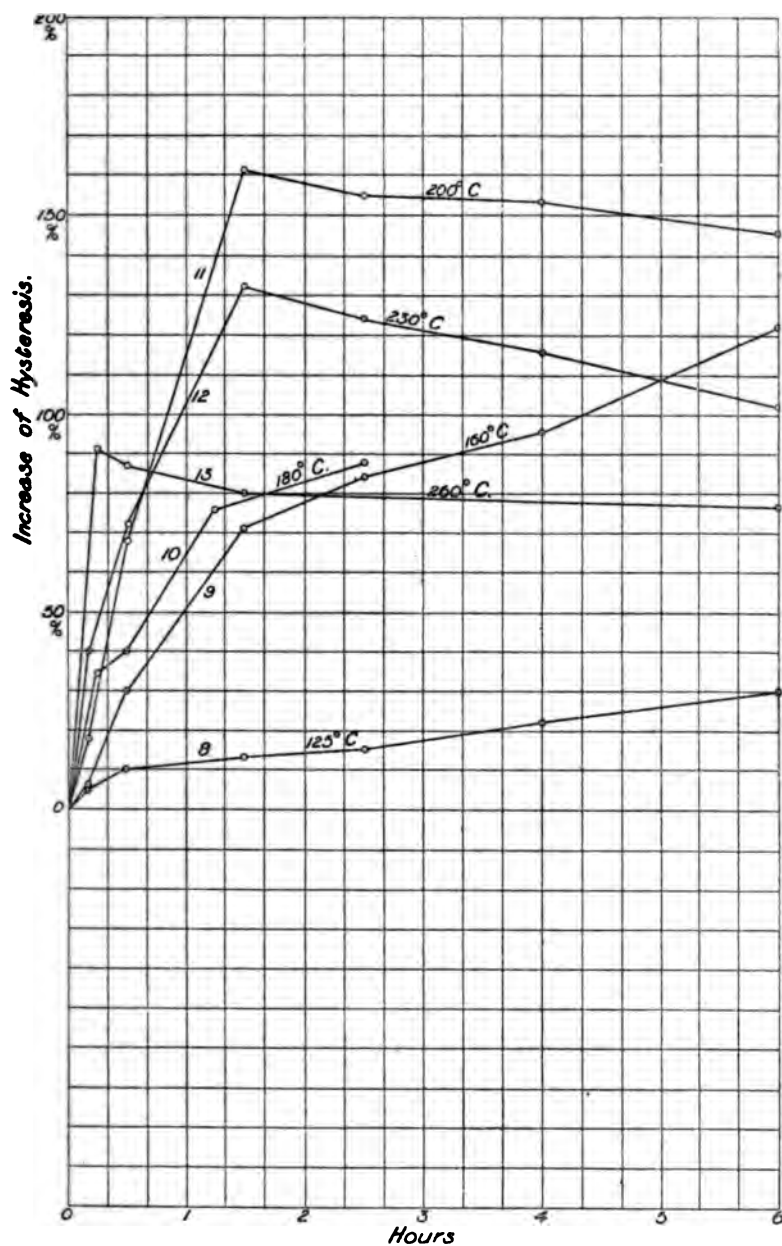


FIG. 2.



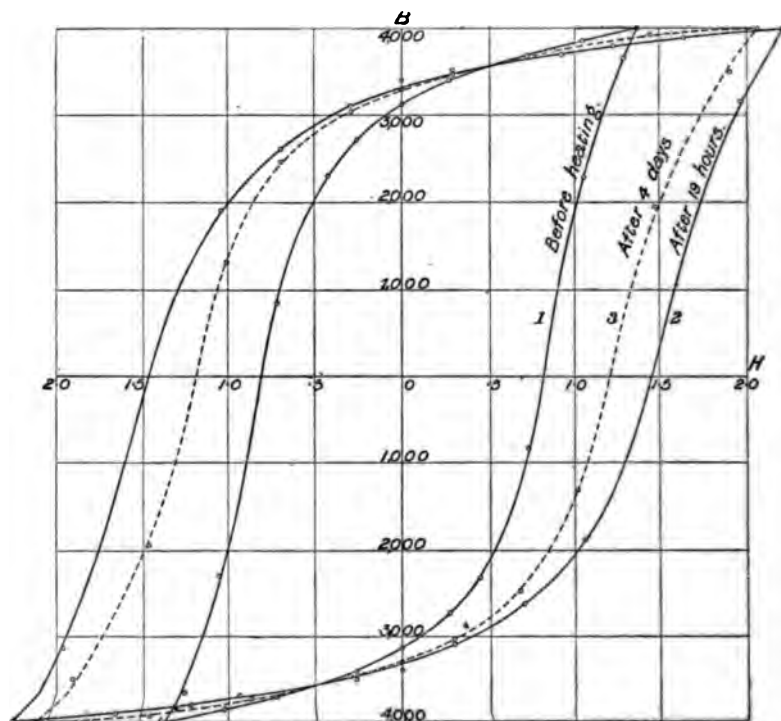
temperatures. This feature in the effect of prolonged heating seems to have escaped the notice of previous workers in the subject.

It appears that there is a temperature in the neighbourhood of 180°C. , for which the maximum increase of hysteresis is greatest. With higher temperatures the hysteresis, although rising more rapidly at first, does not reach so high a maximum value and begins to fall sooner and faster, tending apparently to a lower steady state the higher the temperature. An example of this is shown in curve 7, fig. 1 (temperature 260°C.), where a fairly low and nearly steady state is reached in the last days of the heating. In this instance it took the iron only about a quarter of an hour to reach its maximum of hysteresis, which was only 91 per cent. higher than the initial value.

Fig. 2 shows the earlier stages of the action for temperatures of 125°C. and over. It will be noticed that the peak at which the hysteresis reaches its maximum in each case comes sooner the higher the temperature, and that its height becomes reduced when the temperature is high. The absolute values of the hysteresis in the experiments to which these curves relate are given in Table II.

It is probable that the attainment of a maximum value followed by a decrease is not confined to temperatures above 135°C. , and it

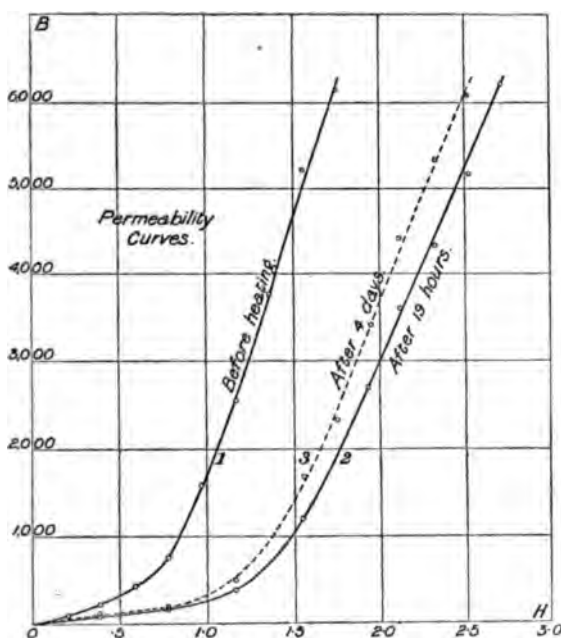
Fig. 3.



is intended to carry out experiments to find if this is so, using more prolonged heating.

In order to exhibit the character of the change in magnetic property, supplementary experiments were made by the ballistic method, using a ring of soft iron formed by coiling up a long strip of sheet metal. This was first annealed, and tested in the annealed state. It was then baked by heating at $200^{\circ}\text{C}.$, and cyclic curves were determined in the usual way after the ring had become cold. The results are stated in Table III, and are shown in the curves of fig. 3. Curve 1 shows the initial state (after annealing), where the value of the hysteresis is 830 ergs per cycle per cubic centimetre ($B = 4000$). Curve 2 shows the state after nineteen hours of baking at $200^{\circ}\text{C}.$, when the hysteresis had greatly increased and had reached a value of 1580 ergs. Curve 3 was taken after further heating at the same temperature for four days, by which time the decrease of hysteresis is very apparent, its value having diminished to 1420 ergs. Permeability curves, taken by the method of reversals after heating during the same periods, are given in fig. 4 and

FIG. 4.



show the falling off and subsequent partial recovery in the permeability.

Table II.—Change of Hysteresis by prolonged Heating; early stages.

Curve No.....	8.	9.	10.	11.	12.	13.
Temperature ..	125° C.	160° C.	180° C.	200° C.	230° C.	260° C.
Time of heating.	Hysteresis.		Hysteresis.		Hysteresis.	
	Abs.	Increase per cent.	Abs.	Increase per cent.	Abs.	Increase per cent.
Hrs. mins.						
0 0	600	0	595	0	570	0
0 10	630	5	630	6	800	40
0 15
0 30	660	10	770	30	980	72
1 15
1 30	680	13	1015	71	1320	132
2 30	690	15	1090	84	1275	124
4 0	730	22	1160	96	1230	116
6 0	800	30	1300	122	1080	102

Table III.—Cyclic Curves for Ring.

1. Before heating.		2. After 20 hours at 200° C.		3. After 4 days at 200° C.	
H.	B.	H.	B.	H.	B.
1·36	4000	2·20	4000	2·06	4000
1·26	3650	1·97	3170	1·91	3500
1·06	2300	1·05	-1900	1·47	1950
0·72	-850	0·70	-2620	1·02	-1310
0·44	-2320	0·28	-3100	0·68	-2480
0·26	-2740	0·00	-3400	0·28	-3050
0·00	-3130	-0·28	-3460	0·00	-3310
-0·26	-3400	-0·87	-3700	-0·28	-3520
-0·72	-3700	-1·23	-3790	-0·87	-3740
-1·05	-3850	-1·51	-3880	-1·45	-3930

Table IV.—Permeability Curves for Ring.

1. Before heating.		2. After 20 hours at 200° C.		3. After 4 days at 200° C.	
H.	B.	H.	B.	H.	B.
0·19	90	0·39	70	0·39	100
0·39	220	0·78	160	0·78	190
0·58	420	1·16	390	1·16	520
0·78	760	1·55	1200	1·55	1660
0·97	1570	1·94	2730	1·75	2320
1·16	2550	2·12	3600	1·94	3400
1·36	3760	2·33	4320	2·12	4420
1·55	5220	2·52	5480	2·33	5340
1·75	6150	2·72	6220	2·52	6100

In order to ascertain whether the effects could be due to the iron taking up anything from the atmosphere, a couple of specimens were, at the suggestion of Professor Ewing, sealed up in exhausted bulbs and heated in the same oven with others exposed to air. No evidence, however, was found of any difference between the iron heated *in vacuo*, and that heated in air.

There is nothing to show that when baked iron is left for a long time it recovers from the condition produced by baking. But in all cases the iron is restored to its original state of low, *hysteresis* by reannealing.

In conclusion the author wishes to express his thanks to Professor Ewing for many suggestions as well as for the facilities which have enabled the experiments to be carried out.

[Subsequent to the writing of the above paper, permeability curves have been taken, for a ring of the same material, by the ballistic method, up to a magnetic force of 25 C.G.S. units. A comparison of those taken before and after baking shows that the saturation value of the induction is unchanged by prolonged heating, for although the earlier part of the curve, as in the case given above, was much altered, the parts of the curves above a force of 15 C.G.S. units are practically indistinguishable.—May 15, 1898.]

“On the Connexion of Algebraic Functions with Automorphic Functions.” By E. T. WHITTAKER, B.A., Fellow of Trinity College, Cambridge. Communicated by Professor A. R. FORSYTH, Sc.D., F.R.S. Received April 23,—Read May 12, 1898.

(Abstract.)

If u and z are variables connected by an algebraic equation, they are, in general, multiform functions of each other; the multiformity can be represented by a Riemann surface, to each point of which corresponds a pair of values of u and z .

Poincaré and Klein have proved that a variable t exists, of which u and z are uniform automorphic functions; the existence-theorem, however, does not connect t analytically with u and z . When the genus (*genre*, *Geschlecht*) of the algebraic relation is zero or unity, t can be found by known methods; the automorphic functions required are rational functions, and doubly periodic functions, in the two cases respectively. But no class of automorphic functions with simply connected fundamental polygons has been known hitherto, which is applicable to the uniformisation of algebraic functions whose genus is greater than unity.

The present memoir discusses a new class of groups of projective substitutions, such that the functions rational on a Riemann surface of any genus can be expressed as uniform automorphic functions of a group of this class. These groups are sub-groups of groups generated from substitutions of period two. Groups are first considered which can be generated by a number of real substitutions of period two, whose double points are not on the real axis, and whose product in a definite order is the identical substitution. These groups are found to be discontinuous, and of genus zero. A method is given for

dividing the plane into curvilinear polygons corresponding to such a group; these polygons are simply-connected, and cover completely the half of the plane which is above the real axis. Sub-groups of these groups are found, whose genus is greater than unity, and which are appropriate for the uniformisation of any algebraic curves.

The sides of the polygons, into which the half-plane is divided, are formed of arcs of circles orthogonal to the real axis. These may, in the sense of Lobatchewski's geometry, be regarded as straight lines. One case, where the construction fails, is shown to correspond to the limiting case in which Lobatchewski's geometry becomes Euclidian geometry; the figure then becomes the division of a plane into parallelograms, used in the theory of doubly periodic functions, and is appropriate for the uniformisation of algebraic curves of genus unity. Thus doubly-periodic functions are a limiting case of the class of functions considered.

The automorphic functions of the groups described solve the problem of conformally representing a plane, regarded as bounded by a number of finite lines radiating from a point, on a curvilinear polygon, whose sides are derived from each other in pairs by projective substitutions of period two. This leads to the conformal representation of any Riemann surface, at each of whose branch-points only two sheets are connected, on a curvilinear polygon whose sides are derived from each other in pairs by projective substitutions; and, as it is known that any algebraic curve can, by birational transformation, be represented on a Riemann surface whose branch-points are all simple, it is seen that the uniformisation of algebraic functions of any genus can be effected by groups of the kind described.

The analytical connexion between the variables of the algebraic form and the uniformising variables is given by a differential equation of the third order. A certain number of the constants in this equation have to be determined by the condition that the group of substitutions associated with the equation leaves unchanged a certain circle. When any arbitrary values are given to these constants the solution of the differential equation is termed a quasi-uniformising variable. The properties of quasi-uniformising variables, and their relation to the uniformising variable, are discussed in the last section of the paper.

"A Study of the Phyto-Plankton of the Atlantic." By **GEORGE MURRAY**, F.R.S., Keeper of Botany, British Museum, and **V. H. BLACKMAN**, B.A., F.L.S., Hutchinson Student, St. John's College, Cambridge, and Assistant, Department of Botany, British Museum. Received March 28,—Read May 12, 1898.

(Abstract.)

The authors record their observations on a year's work in collecting phyto-plankton along a track from the Channel to Panama carried out by Captains Milner and Rudge, and also during one voyage to Brazil by Captain Tindall. They also give the results of their own observations on living material at sea. The material was obtained by the pumping method.

One of the objects of their work was to determine, if possible, the nature of the Coccospheres and Rhabdospheres. They describe the minute structure of the calcareous plates or coccoliths and rhabdoliths, and record the existence in the Coccospheres of a single central green chromatophore, separating into two on the division of the cell. They regard Coccosphæracæ as a group of Unicellular Algæ, and they define the group, the limits of the genera and species. The Coccospheres and Rhabdospheres from the surface are compared with those of the deep-sea deposits and their identity established. They are also compared with geological coccoliths and rhabdoliths from various beds, and many objects regarded by geologists as true coccoliths and rhabdoliths are rejected. A large number of new Peridiniacæ were discovered and are formally described and figured. No specific diagnoses of marine Peridiniacæ have previously been published, authors of species having depended on figures, and, at most, a few words of description. It is hoped that the present systematic treatment of the subject will conduce to greater order in the group. The authors record the occurrence of all the forms in seven tabular statements, one for each collecting voyage.

Observations of the diatoms and Cyanophycæ were also made, and are briefly treated.

A study was also made of the species of *Pyrocystis*, of which they describe a new one. The facts they record tend, in their opinion, to confirm the view originally expressed of it by Dr. John Murray, its describer, that it is a unicellular alga, doubts having been entertained of the accuracy of this opinion by several biologists.

May 26, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The following Papers were read :—

- I. "On the Cytological Features of Fertilisation and related Phenomena in *Pinus silvestris*, L." By V. H. BLACKMAN, B.A., F.L.S. Communicated by FRANCIS DARWIN, F.R.S.
- II. "The Skeleton and Classification of Calcareous Sponges." By G. P. BIDDER. Communicated by ADAM SEDGWICK, F.R.S.
- III. "On Surfusion in Metals and Alloys." By W. C. ROBERTS-AUSTEN, C.B., F.R.S.
- IV. "Note on the Complete Scheme of Electrodynamical Equations of a Moving Material Medium, and on Electrostriction." By J. LARMOR, D.Sc., F.R.S.
- V. "Aluminium as an Electrode in Cells for Direct and Alternate Currents." By E. WILSON. Communicated by Professor HOPKINSON, F.R.S.
- VI. "Contributions to the Study of 'Flicker.'" By T. C. PORTER. Communicated by Lord RAYLEIGH, F.R.S.
- VII. "On the Kathode Fall of Potential in Gases." By J. W. CARSTICK. Communicated by Professor J. J. THOMSON, F.R.S.

The Society adjourned over the Whitsuntide Recess to Thursday, June 9.

"On the Intimate Structure of Crystals. Part I. Crystals of the Cubic System with Cubic Cleavage." By W. J. SOLLAS, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford. Received January 20,—Read February 3, 1898.

The remarkable advance in our knowledge of the constitution of gases that has marked the latter half of this century has immensely strengthened a belief in the doctrine of atoms. It is to this doctrine, therefore, that we naturally turn to assist us in the study of the intimate structure of solids. The solid, however, stands at the opposite pole to the gas; in the one the particles are unrestrained, free to move about any axis, and along paths of comparatively wide

range, before exerting mutual action on each other; in the solid, on the other hand, they are so closely packed that in many cases the abstraction of heat is accompanied by only a very inconsiderable approach of the atoms to nearer proximity. Thus among metals, that having one of the highest coefficients of expansion is indium; but this only contracts 0.00004 of its bulk for every degree centigrade that it is lowered below the ordinary atmospheric temperature; nor does it continue to contract at this rate, but at a continually diminishing one, as its temperature is progressively reduced. A metal with a very low coefficient of expansion is iridium, which loses only 0.000006 of its volume for every degree that it falls in temperature. Among non-metallic elements we may point to the remarkable case of the diamond, which, with the small coefficient of 0.00000118, actually ceases to contract at all when cooled below 42° C. It may be possible by other means to effect greater reductions in the volume of solids, but so far as the withdrawal of thermal energy is concerned, it evidently accomplishes but very little, at least in the case of solids treated at a temperature considerably below their fusion points.

The assumptions made in the study of gases are of the fewest and simplest kind, but they include one that is of fundamental importance in the investigation of solids, viz., that in some sense an atom is a body occupying space; it is only by making this admission that deviations from important laws like Boyle's find an explanation.

The dominion of an atom over a certain region of space endows that space with form; what that form is we do not know, though we may eventually discover. In the absence of knowledge it is permissible to make some kind of assumption, and then to endeavour to discover how far the consequences of that assumption accord with ascertained facts. I propose to make the simplest assumption possible, and to regard the volume of space appropriated by an atom as having the form of a solid of revolution, and very generally of a sphere. Within this space there exists something, the energy of the atom, or a force of repulsion, or both, which preserves it from invasion by other atoms; outside it there is something, a pressure or force of attraction, which drives the atoms as close together as possible without causing interpenetration. It will probably be found also that these influences, pressures, and thrusts, are directed, or that the atoms are polar.

Dalton's law of multiple proportions seems to meet with its counterpart in crystallography in Haüy's law of "the rationality of the indices," and the conception by which Haüy sought to explain this and other facts relating to the form and structure of crystals presents singular points of resemblance to Dalton's ideas of atoms. Haüy's views on crystalline structure appear to me to contain the

germ of a great truth, which subsequent refinements in the geometrical treatment of the subject have to some extent obscured.

Recent work by Lord Kelvin, Mr. Tutton, and others, has given great encouragement to those who cherish a hope of ultimately arriving at some sure representation of crystalline structure; but the remarkable observations of Penfield* on the several species of the mineral chondrodite are of an especially suggestive character. The chondrodites present us with a homologous series of chemical compounds, having the following constitution:—

	No. of atoms of Mg.
(a) $\text{Mg}_5[\text{Mg}(\text{F.OH})]_2(\text{SiO}_4)_2$	5
(b) $\text{Mg}_7[\text{Mg}(\text{F.OH})]_2(\text{SiO}_4)_3$	7
(c) $\text{Mg}_9[\text{Mg}(\text{F.OH})]_2(\text{SiO}_4)_4$	9

As Professor Miers tells us, "the three minerals have almost the same form, but differ only in the length of one crystallographic axis (parameter). The lengths of this . . . in the three minerals are in the ratio of 5 : 7 : 9. Thus the addition of the olivine radicle, Mg_2SiO_4 , exerts a so-called 'morphotropic' action along one definite axis, and causes a certain constant increase in the length of this axis (parameter)." "It is further a curious fact that the length of this (parameter) varies directly as the number of magnesium atoms in the compound." The inference that the magnesium atoms are disposed along this axis would seem to follow naturally.

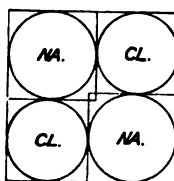
Mr. Barlow has lately completed an investigation into the different ways in which spherical bodies of different volumes may be packed together so as to produce different crystal forms. His method of treatment is general, and many of the arrangements of atoms, which I am myself led to imagine as existing in certain crystals, will, no doubt, be found to fall as special cases under his general laws. But I do not propose in this communication to merely suggest some way in which atoms regarded as spheres may be built up into geometrical forms; my purpose is different, it is to consider certain particular substances, and to endeavour to discover the particular structure which must most rationally be assigned to them. Further, to avoid misconception, it should be added that the "law of closest packing," which is so admirably developed by Mr. Barlow, does not seem to be obeyed by the greater part of the crystals I have investigated; so far as the cubic system is concerned, there is but one simple substance in which the atoms are as closely packed as possible, that is the diamond.

It is a singular fact that crystals of the cubic system, when composed of salts which are diatomic, such as NaCl , KCl , are characterised by a cubic cleavage, while those composed of triatomic salts

* 'Amer. Journ. Science,' vol. 47, p. 188.

such as cuprous oxide (Cu_2O), or fluorspar (CaF_2), possess an octahedral cleavage. Let us for the moment follow the hint given by Haüy, and regard the cubic crystals with cubic cleavage as reduced by repeated cleavage to a primitive cubelet. How in the case of a diatomic compound composed of atoms of different relative sizes, as, say in common table salt, can the molecules be arranged together so as to produce this cubelet?

FIG. 1.



The only satisfactory way that I can discover is to place one molecule (NaCl) by the side of another (ClNa), as in the diagram (fig. 1), so that tangent planes to the pairs of atoms may form a square prism, and then to rest on the top of this two more molecules, similarly put together, the chlorine of the upper group resting on the sodium of the lower, and the sodium of the upper on the chlorine of the lower; common tangent planes to the exterior of the spheres taken in fours will be found to form a cube.

This is a simple and possible arrangement of the molecules of common salt, but whether it alone and not some other is the actually existent arrangement is a question for argument.

Ever since Kopp formulated his famous laws, the subject of atomic volumes has possessed great interest for the chemist, though the discrepancies which too frequently impair the correspondences discovered by Kopp have evoked a general feeling of mistrust.

Nevertheless I am persuaded that in atomic volumes we possess the touchstone by which our conjectural arrangements may be brought to the test. The study of atomic volumes and crystalline structure cast mutual light upon each other.

Kopp obtained his volumes by dividing the molecular weight (m) of a substance by its density (d), thus $m/d = v$. Evidently if atoms have the form of solids of revolution, this volume is a lump quantity, including not only the volume of the atoms, but of the interstices between them. We shall therefore speak of it as the gross volume (V), in contradistinction to the true relative volume (v) of the atom.

The shortest way of explaining our method of investigation will be to describe at once the process as applied to the haloid compounds of the alkali metals. Let us commence with common table salt. Its

molecular weight is 58.513 (Na 23.06 + Cl 35.453).^{*} Its density, corresponding to the specific gravity, found as the result of very careful determinations by Retgers, is 2.167 at 17° C. The gross volume is then $58.513/2.167 = 27.0002$. If this number be multiplied by 4, it will give the volume of a primitive cubelet, supposed to be built up of 4 molecules, as already described. The cube root of this number (108.01) is 4.7623, and represents the length of an edge of the cubelet, or the sum of the diameters of the atoms of sodium and chlorine, by which this edge is determined. The next step is to endeavour to obtain some probable value for the ratio of these diameters, that of the sodium to that of the chlorine atom.

It is quite certain if the sodium and chlorine atoms do not both occupy the same space, that the volume of sodium, and the other alkali metals, is very different in the free state and in combination. This is a conclusion to which Kopp was led, who found that by assuming a reduction of the volume in the free state to one-half when the element entered into combination, he obtained fairly constant differences on subtracting the half volumes of the metals from the volumes of the chlorine salts. Our investigations support this opinion of Kopp. In the following table data for investigation are given for such haloid compounds as are open to study.

Metals.	<i>m.</i>	<i>d.</i>	V.	T.	F.
Lithium	7.03	0.578	12.162	—	186.0°
„	—	0.589	11.936	—	—
Sodium	23.06	0.984	23.46	0°/4°	95.6
Potassium	39.14	0.865	45.25	15°	62.0
„	—	0.875	44.732	13°	—

m, atomic weight; *d*, density; V, gross volume; T, temperature at which sp. gr. was determined; F, fusion point.

Salt.	<i>m.</i>	<i>d.</i>	V.	T.
Lithium chloride....	42.483	2.074	20.484	3.9
Sodium chloride	58.513	2.167	27.0002	17.0
Potassium chloride..	74.593	1.989	37.7503	16.0
Lithium bromide....	86.993	3.102	28.004	17.0
Sodium bromide	103.023	3.079	33.404	17.0
„	—	3.198	32.217	17.3
Potassium bromide ..	119.103	2.7	44.12	—
Lithium iodide	133.89	3.485	33.218	23.0
Sodium iodide	149.92	3.654	41.029	18.2
Potassium iodide....	166.0	3.059	54.266	—
„	—	3.079	53.914	—

^{*} In this and all subsequent cases the atomic weights are those given by Ostwald to oxygen, 16, as a base.

With the exception of the densities of sodium and potassium chlorides, which have been very exactly determined by Retgers, the densities given above cannot be regarded as more than approximations, not to be trusted in the second place of decimals, not always in the first. If one-half of the volumes of the metals given in the first table be subtracted from the volumes of the salts given in the second, we obtain the following results:—

Salt.	V.	$\frac{1}{2}$ V. Metal.	V. Haloid elements.
Lithium chloride	20·484	5·968	14·516
" 	—	6·08	14·404
Sodium chloride	27·002	11·73	15·272
Potassium chloride ..	37·7503	22·625	15·1253
Lithium bromide	28·004	5·968	22·032
" 	—	6·08	21·92
Sodium bromide	32·217	11·735	20·487
Potassium bromide ..	44·112	22·625	21·487
Lithium iodide	138·318	6·07	32·23
Sodium iodide	41·029	11·73	29·299
Potassium iodide	53·914	22·625	31·289
" 	54·266	—	31·641

It will be observed that the difference between the gross volume of the salts containing the same haloid element and the gross volume of the metal they contain is approximately equal in all cases; thus, the common difference in the case of the chlorides varies from 14·4 to 15·27; in the case of the bromides, from 20·487 to 20·032; in that of the iodides, from 32·23 to 29·299. It was assumed by Kopp that these differences corresponded to the volume of the haloid element. We make the same assumption, and by treating the subject from another point of view obtain a more consistent agreement between the differences which represent the volumes of the haloids.

The system in which lithium bromide crystallises is not known; the same is true of lithium iodide, and these salts are in consequence necessarily excluded from our inquiry. Neglecting them, it will be seen by inspection that the greater the difference between the gross volume of metal and the haloid with which it is combined, the smaller is the value found for the volume of the haloid; thus, in the case of the chlorides, the smallest value found for chlorine is 14·4, and the disparity between the volumes of lithium and chlorine is the greatest in the series. In the same way the bromine in sodium bromide has a less volume than that in the corresponding potassium salt, and the volumes of sodium and bromine are farther removed from equality than those of potassium and bromine. This is a necessary consequence of such an arrangement of spheres as

we have imagined to exist in the case of common salt, already described, for it can readily be shown, and will presently appear, that the ratio of the volumes of the eight spheres forming a primitive cubelet to the volume of the interstices continuously diminishes as the spheres approach equality, or, more precisely, the total volume of the cubelet, including interstices, is to the volume of the contained spheres as 1.9099 : 1 when the spheres are all equal, and diminishes from this down to a limit, of which we shall have more to say presently, of 1.7125 when the two kinds of spheres have attained the greatest possible degree of inequality consistent with the arrangement assigned to them.

Of the different possible modes of packing in the cubic system, the one we have adopted is the only one that gives this result, within the limits of difference in size which we are considering; every other kind of simple packing leads to an increase in the gross volume when the two sets of constituent spheres depart from equality in dimensions. There is but one exception to this statement, and this does not affect our argument, as it is true of one particular ratio only, and is not applicable to the haloid salts of the alkalis.

Returning now to the imagined cubelet of common salt, we are able to give a value to the diameter of the sodium atom it contains; thus, the gross volume of the sodium is 11.73, one-half of the gross volume of sodium in the free state; suppose eight atoms of sodium built up into a cube in the same way that the four molecules of sodium chloride were imagined to be built up, one sphere to each corner of the cube, then the edge of the cube will be equal in length to the sum of the diameters of two sodium atoms. Thus, the gross volume of sodium $11.73 \times 8 = 93.84$, the gross volume of eight atoms, and $\sqrt[3]{93.84} = 4.5443$, the length of the edge of the cube; this divided by 2 gives the diameter of a sodium atom as 2.2721.* But the length of the edge of a cubelet of sodium chloride was found to be 4.7623; deducting the diameter of a sodium atom from this, we have $4.7623 - 2.2721 = 2.4902$, the diameter of an atom of chlorine.

Using the value found for the diameter of an atom of sodium as a basis, we may proceed to treat all the haloid salts available for examination in the same way as follows:—

	Diameters of atoms.		Diameters of atoms.
LiCl.....	4.3433	KCl.....	5.3216
—Cl.....	2.4902	—Cl.....	2.4902
	<hr/>		<hr/>
Li =	1.8531	K =	2.8314

* This could of course be directly obtained from the gross volume, which might be regarded as the cube circumscribing the sodium sphere; but I am anxious to preserve a parallelism in treating the salts and the elements.

	Diameters of atoms.		Diameters of atoms.
NaBr	5·051	KBr	5·6089
—Na	2·2721	—K	2·8314
	<hr/>		<hr/>
Br =	2·7789	Br =	2·7775
NaI	5·4750	KI	6·0099
—Na	2·2721	—K	2·8314
	<hr/>		<hr/>
I =	3·2029	I =	3·1785

These results, however, are merely approximate,* for, as Professor Miers has pointed out to me, the diameter of the second sphere in a cube cannot be obtained by simply subtracting the diameter of the other from the edge. Professor Miers shows that the relation between the length of the edge of the cube (A) and of the radius of the larger spheres (R) and of the smaller (r) may be expressed by the equation—

$$(R+r)^2 = [A-(R+r)]^2 + [\sqrt{2}(R-r)]^2,$$

from which he obtains—

$$R = \frac{1}{2} \{2r + A \pm \sqrt{(8Ar - A^2)}\},$$

or

$$r = \frac{1}{2} \{2R + A - \sqrt{(8Ar - A^2)}\}.$$

Employing this formula, the diameters of the atoms are found to be as follows :—

Lithium chloride	Li	1·8848	Cl	2·4954
Sodium chloride	Na	2·2721	Cl	2·4954
Potassium chloride	K	2·8372	Cl	2·4954
Sodium bromide	Na	2·2721	Br	2·807
Potassium bromide	K	2·8372	Br	2·772
Potassium iodide	K	2·8372	I	3·1957
Sodium iodide	Na	2·335	I	3·1957

There does not appear to be any sound reason why the volume of an element should remain absolutely constant and independent of the element with which it is associated, even in a homologous series, and I am inclined to think that the differences which appear in our investigation are not wholly due to defects in our knowledge of the specific

* It has since occurred to me that a slight modification in our conception of the manner in which the atoms are arranged will render our determinations exact; this will be the case if we consider the atoms to be situated with their centres on the nodes of a cubic lattice; we shall recur to this in a subsequent part of this communication.

gravity of the different salts, but that they are real and of significance. The difference in the coefficients of expansion for the different haloids suggest a certain want of constancy in volume. Thus, Fizeau obtained for the linear coefficient of expansion of

		$\Delta\alpha/\Delta\theta$.
NaCl	0.00004039	4.49
KCl	0.00003803	5.15
KBr	0.00004201	9.78
KI	0.00004265	16.76

and since it is improbable that the component atoms of these salts all possess the same coefficient of expansion, it would seem that an absolute constancy of relative volume is in a high degree improbable. It is sufficient, however, for our purposes if it can be shown that the deviation from constancy is confined to narrow limits, as indeed, from the results so far obtained, appears to be the case.

The importance of these results for our inquiry is, however, this, that they cannot be obtained by any other method of packing; other methods, instead of minimising the differences between the gross volumes, would, on the contrary, exaggerate them.

The relative dimensions of the atoms already considered may be tabulated as follows :—

Element.	Diameter.	Volume.	Element.	Diameter.	Volume.
Li	1.8848	3.5059	Cl	2.4954	8.1274
Na	2.2721	6.1408	Br (in KBr)	2.772	11.1526
„ (in NaI)	2.325	6.6659	„ (in NaBr)	2.807	11.5804
K	2.8372	11.960	I	3.1957	17.1276

In the next table the sum of the volumes of the atoms in a molecule is compared with the gross volume of the salt, and exhibited in the form of a ratio.

Salt.	I.	II.	III.
LiCl	11.6333	20.484	1 : 1.7608
NaCl	14.2682	27.0002	1 : 1.8923
KCl	20.0874	37.7503	1 : 1.8793
NaBr	17.7212	32.217	1 : 1.818
KBr	23.1126	44.112	1 : 1.9085
NaI	23.7935	41.029	1 : 1.7244
KI	29.0876	54.266	1 : 1.8656

I. Sum of the volumes of the atoms.

II. Gross volumes of the salts.

III. Ratio of I to II.

It will be observed that in every case the ratio is smaller the greater the difference between the size of the component atoms.

It may next be pointed out that there exists a very important limitation to our power of arranging pairs of atoms or diatomic molecules in the manner we have suggested; the two atoms of the molecule may be equal in size, or they may be unequal, so long as the inequality does not exceed the value of $1 : 0.7286$.*

I owe to the kindness of Professor Miers the following method of finding the value of this limit. Let the centres of the spheres be referred to three edges of the cube, meeting at one corner, as the axes x , y , and z . The coordinates for the centre (C_1) of the larger sphere are $A-R$, R , R , and for the centre C_2 of the smaller sphere r , r , r . Then

$$(C_1C_2)^2 = (A-R-r)^2 + 2(R-r)^2.$$

In the limiting case, when the two larger spheres are in contact,

$$A = R(2 + \sqrt{2}),$$

$$\therefore (C_1C_2)^2 = R^2(5 + 2\sqrt{2}) - 2Rr(3 + \sqrt{2}) + 3r^2.$$

Also

$$(C_1C_2)^2 = (R+r)^2,$$

$$\therefore R^2(4 + 2\sqrt{2}) - 2Rr(4 + \sqrt{2}) + 2r^2 = 0.$$

$$\left(\frac{R}{r}\right)^2 = 2 \frac{R}{r} \cdot \frac{5.4142}{6.8284} + \frac{1}{3.4142} = 0.$$

$$\therefore R/r = 0.7929 \pm \sqrt{0.3358} = 1.3724 = 1 \div 0.72865.$$

When the smaller spheres fall below the limiting value 0.729 the tangent planes to the spheres no longer define a cube, but a figure which is a hemimorphic form of the rhombohedral or hexagonal system; and in all cases that I have yet examined of diatomic salts, belonging to a homologous series crystallising in the cubic system with cubic cleavage, I find that directly one of the atomic volumes falls below this limit the salt passes out of the cubic system and presents itself under hemimorphic hexagonal forms. This is the case with silver iodide, which is fully discussed in the second part of this communication. That the substances to which we have at present restricted our attention consist of pairs of atoms which lie within the prescribed limit is shown by the following table, in which the ratios of the diameters are

* If the centres of the spheres be situated on the nodes of a cubic lattice, the value of this limit will be changed.

$\frac{\text{Li}}{\text{Cl}} = \frac{1.8848}{2.4954} = 0.7556.$	$\frac{\text{Na}}{\text{Cl}} = \frac{2.2721}{2.4954} = 0.9108.$	$\frac{\text{Cl}}{\text{K}} = \frac{2.4954}{2.8372} = 0.8792.$
$\frac{\text{Na}}{\text{Br}} = \frac{2.2721}{2.807} = 0.8094.$	$\frac{\text{Br}}{\text{K}} = \frac{2.772}{2.8372} = 0.977.$	$\frac{\text{Na}}{\text{I}} = \frac{2.2721}{3.1957} = 0.7107.$
$\frac{\text{K}}{\text{I}} = \frac{2.8372}{3.1957} = 0.8878.$	Maximum limiting ratio, 1 : 0.72865.	

If the arrangement of the atoms in the crystals under consideration be, as I conceive, the actually existing arrangement, it is certainly not the one to which views on close packing would have led us. Nature does not appear to have been at all parsimonious of space, and that atoms should be disposed in such comparatively open order and yet produce structures of great rigidity, almost inevitably suggests the existence of poles or directed forces. The importance of the question renders it necessary to probe the matter deeper, and we may naturally seek for further confirmation of our hypothesis. Since the arrangement at which we have arrived is the most open probable, we may expect if any other arrangements exist to find evidence of closer packing. We turn, therefore, to the evidence afforded by solutions. In a solution the molecules are not, as a rule, constrained to oscillate about fixed positions, they are not built up into a solid architecture, but are free to glide over one another, and to migrate from place to place. In their case open packing is not to be expected; in the absence of arrangement, the closest packing will prevail. Closest packing among equal-sized spheres exists when one is surrounded by twelve others in contact. It is the arrangement met with in triangular piles of round shot. In a stack of this kind the ratio of the volume of the balls to the volume of the pyramid they form, including interstices, is as 1 : 1.35.

On introducing a crystal of common salt into water the crystalline edifice is destroyed, and the separated molecules become surrounded by those of the solvent in closest packing. As the solution is made very dilute the molecules of the salt are resolved into their ions, which wander about in the solution, still under the influence of internal pressures, adapting themselves to the law of closest packing.

Under these circumstances what change of volume is naturally to be expected when common salt passes into brine? Clearly a contraction, and that to a considerable amount, such, indeed, as is always actually observed whenever the haloid salts of the alkalis are dissolved in water. This general observation affords strong confirmation of the truth of our hypothetical arrangement, which will be still further strengthened if we pursue the subject into quantitative comparison. The ideas involved in the expression atomic volume are less simple in the case of a liquid than in that of a solid. In any case the atomic volume must be regarded as an

average effect, but in a liquid there are more factors to be integrated. The results of the following investigation are necessarily, therefore, only first approximations.

From the numbers which we have already assigned to the relative volumes of the atoms the relative density follows from the relation $m/v = d$, and by dividing d by the factor 1.35 we obtain the density of the substance, which would be produced by the atoms, when most closely packed together. Data are given in the following table:—

Element.	Atomic weight.	Volume of atom.	Density.	$D \div 1.35$.
Li.....	7.03	3.5059	2.0009	1.4853
Na.....	23.06	6.1408	3.7552	2.7816
„ in NaI...	..	6.6659	3.4594	2.5625
K.....	39.14	11.960	3.2727	2.4243
Cl.....	35.453	8.1274	4.3622	3.2312
Br.....	79.963	11.1526	7.17	5.311
„ in KBr	11.5804	6.905	5.1148
I.....	126.86	17.1276	7.4238	5.4991

The researches of chemists have given us exact determinations of the specific gravity of solutions of the haloid salts of the alkalis, and from these we may calculate the density of the ions. From the equation A/d_1 , $B/d_2 = 100/D$, where A and B represent the percentage of salt and water respectively, d_1 and d_2 the density of each, and D the density of the solution. The specific gravity of the solutions examined will be found given in Watts' 'Dictionary of Chemistry,' or Whetham's 'Solution and Electrolysis,' and need not be repeated here; they are derived from Gerlach's tables, published in 1869, in the 'Zeitschrift für Analytische Chemie,' vol. 8, p. 245. Wherever possible, I have selected for examination a solution containing a gram-molecule of the salt to a litre of water. Making d_1 in the equation above = x , its value, as found for the different haloid salts, is given in the table below:—

Salt.	Density of molecule.	Density of molecule $\div 1.35$.	Density found from solution.	Number of gram-molecules in 1 litre of solution.
LiCl.....	3.6518	2.7051	2.5585	1.0
NaCl.....	4.0741	3.0178	3.4605	1.0
KCl.....	3.7134	2.7507	2.6731	1.0
NaBr	5.8104	4.3262	3.938	0.9687
KBr.....	5.1532	3.8172	3.4916	0.4198
NaI.....	6.3000	4.6673	4.1118	0.721
KI	5.7069	4.2273	3.5976	1.0

As close a correspondence appears as could be expected from the conditions of the case, and it would seem that the crystalline structure attributed to the haloid salts, while inconsistent with no known group of facts, is in quantitative accordance with all that we have investigated, and throws unexpected light on hitherto recondite phenomena. The subject of solution must not, however, be left without giving the densities found for more dilute solutions: they are shown in the following table:—

Salt.	Gram-molecules per litre.	Specific gravity of solution.	Calculated density of molecule.
LiCl.....	0·1	1·0021	3·6093
NaCl	0·1	1·0038	5·557
KCl	0·1	1·0046	3·9989
NaBr	0·4854	1·04	4·7223
KBr	0·072	1·0063	3·488
NaI.....	0·346	1·0374	3·993
KI.....	0·1	1·0112	4·2709

Some of these numbers are less concordant with the density deduced from the crystalline structure than those obtained with stronger solutions, as presented in the preceding table, but it is to be remarked that whatever errors exist in the observations are all thrown on to the values we have obtained; the numbers given in the third column bear all the burden of error arising when dealing with very minute quantities. To show how greatly our results are influenced by slight variations in specific gravity, as given in the second column of numbers above, I have calculated backwards from the density of the atoms to find what specific gravity should give us identical values for the density, as calculated from the crystalline structure and from solution. It is not necessary to give the results for more than two salts; in the case of potassium chloride the specific gravity of the solution, to accord with theory, should be 1·0038 instead of 1·0046 observed, to reduce the density 3·9989 to 2·7668, its value on the assumptions we have made; in the case of sodium chloride the specific gravity should be 1·0019 to give a density of 3·0467, instead of the 1·0038 found. Although different observers do not always give the same specific gravity for the same solutions of salts, it is certain that the discrepancies between theory and observation before us are not to be explained away by blaming the observations; we have before us an interesting residual phenomenon, susceptible of more explanations than one.

It will be observed that those salts the density of which is in excess of that predicted, are those whose volumes are relatively

small. It is only when the ions possess a volume identical with that of the molecules of water that exact agreement between theory and observation can result. The molecular volume of water at 15° is $0.9987/18.006 = 18.083$, and if its molecules are closely packed, the true molecular volume will be obtained by dividing this number by 1.35; this gives 13.395; the diameter of the molecular volume is 2.9465. Comparing the diameter of the ions with this, taken as unity, we have:—

Water*.....	1.0
Li	0.6289
Na.....	0.7711
K	0.9609
Cl	0.8259
Br.....	0.9472
I.....	1.0828

It will be seen that the ions of potassium bromide and iodide make the nearest approach to equality with the atomic volume of water, and should consequently give the most accordant results with theory; this will be found to be the case on reference to the tables, the differences which appear are well within the limits of experimental error. The chlorides are all characterised by giving to theory a density which is in excess.

Alternative explanations may be offered; on the one hand, the molecular volume of water has been taken as spherical, because its molecules are most probably in rotation, but it is quite within the bounds of possibility that the ions in their migrations may roll upon the constituent atoms of the water molecules, and thus to some extent invade the boundary of the molecular volume.

In the case of the smaller ions with relatively small volume this would lead to a considerable increase in the calculated density, while in the case of the larger ions it would be scarcely affected. It is also possible when the commingled spheres of the ions and water molecules are of a diameter bearing a certain ratio to each other that closer packing may be brought about, so that the volume of interstices we have allotted to the smaller ions would be reduced; this is a question for geometers.

The crystallographic study of these salts remains for consideration. There are two ways in which the primitive cubelets may be arranged in the construction of crystals: they are shown in plan in figs. 2 and 3; both give homogeneous assemblages in the cubic system, but that shown in fig. 3 is holohedral, that in fig. 2 is hemihedral.

* *March 4.*—But, as Ramsay and Shields have shown, the molecule of liquid water includes four molecules of the formula H_2O ; the difference between its dimensions and those of the ions is therefore greater than is given here.

FIG. 2.

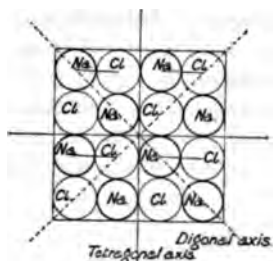
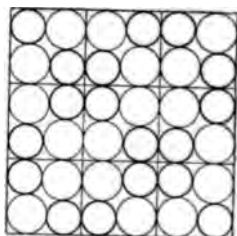


FIG. 3.



When, as until recently, sodium chloride was assigned a holohedral symmetry, it would have been natural to suppose that the arrangement of its molecules was that of fig. 3; but lately it has been placed among hemihedral crystals with which potassium chloride has long been associated. The structure of these salts, and judging by analogy of most of the alkaline haloids, is thus that represented by fig. 2.

It is of interest to observe that in the holohedral arrangement similar atoms are brought into contact; this is not the case with hemihedral symmetry. Perhaps in this is to be found an explanation of the fact that diatomic compounds of monads do not possess holohedral symmetry.

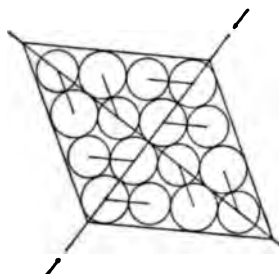
A plagiohedral asymmetry is revealed in the structure both of common salt and potassium chloride by the action of solvents, which produce etch-figures bounded by faces of a hexakis-octahedron. The factors which determine the forms produced by solution are unknown, but probably include the disposition of the chemical bonds of the atoms in the molecule, and the relation of the water molecules to the disposition of the atoms in the crystal. It is evident, however, both from models and figure that there is a skew in the distribution of the chemical bonds of our molecules, and this may be connected with the skew faces produced by solution.

Planes can be drawn through the assemblage of fig. 2 parallel to the faces of the cubelets without crossing any of the molecules; these may be regarded as planes of cleavage.

When a cube of rock salt is truncated at two opposite edges, parallel to planes of a rhombic dodecahedron, and pressure is exerted on the crystal in a direction normal to these faces, a persistent compression results in the direction of the diagonal, and accompanying this a doubly refracting stripe appears. With increased pressure a clean fracture is produced, parallel to one of the rhombic dodecahedral faces to which pressure is applied.

All this is in complete consistence with the structure hypothetically assigned to the crystal. The effect of pressure, as I imagine it,

FIG. 4.



is shown in the diagram (fig. 4), the molecules are forced to slide over each other; those lying on the diagonal along which pressure is exerted make a nearer approach to each other; those at right angles are driven further apart. Up to the critical point, compression and accompanying anisotropy are produced; beyond it the gliding plane becomes a plane of fracture.

The question of elasticity must be left to elasticians; but I would venture to point out the facts elicited by Voigt and Koch; Voigt found for the elasticity modulus of rock salt a coefficient of 4170 kilograms per square millimetre parallel to the tetragonal axes or edges of the cube, 3400 kilograms parallel to the digonal axes or normal to the rhombic dodecahedral faces (110), and 3180 kilograms parallel to the trigonal axes or normal to the octahedral faces (111). Koch found for potassium chloride, 4009 kilograms parallel to the tetragonal axes, and 2088 kilograms along the digonal axes. Voigt obtained very similar quantities. Looking at the plane of assemblage given in the figure (fig. 2), one would suggest that these results are of a kind to be expected; the force exerted along the tetragonal axis is chiefly effective in producing compression of the atoms, along the diagonal axis in distorting the structure. An exact correspondence is obtained by Lord Kelvin between the theoretical elasticities of a hypothetical cube and the results obtained by direct measurement of potassium chloride, but the correspondence does not extend to the results of observation for rock salt, and the structure we have assigned to our crystals is not that considered by Lord Kelvin in his investigation.*

The hardness of rock salt in different directions has been investigated by Exner, who finds that its value is at a maximum parallel to the edge, and at a minimum parallel to the diagonals, of the face of a cube. This result was obtained by determining the weight, with which it was necessary to load a finely pointed needle, to cause it to

* "On the Elasticity of a Crystal according to Boscovich," 'Roy. Soc. Proc.,' vol. 54, p. 69.

scratch the face of the crystal. It was found that if it required a load of one to produce scratching parallel to the edge of a face of the cube, a load of 1·3 was necessary parallel to a diagonal. Looked at broadly, it will be seen by reference to the figure that the effect of a force acting parallel to a diagonal should be to that acting parallel to a side, as $1 : \sqrt{2}$, *i.e.*, as $1 : 1\cdot414$. Considering how complicated the problem actually is, this correspondence is quite as close as could possibly be expected.

By several distinct lines of argument, resting on the study of molecular volumes, of the density of solutions, of the symmetry and physical characters of crystals, we have been led to the same result; given the atomic volumes in the ratios we have assigned to them, no other was possible; and we may now with greater confidence proceed to the investigation of other diatomic compounds, which will be found to throw a surprising light on the molecular tactics of crystals.

"On the Intimate Structure of Crystals. Part II. Crystals of the Cubic System with Cubic Cleavage. Haloid Compounds of Silver." By W. J. SOLLAS, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford. Received January 27,—Read February 3, 1898.

The haloid compounds of silver offer more points of interest in the study of crystal tactics than those of the alkalis, especially as they include that remarkably anomalous substance—iodide of silver.

Silver itself crystallises in the cubic system, and possesses the same kind of structure as all other metals which possess cubic symmetry, *i.e.*, its atoms are arranged on the plan of most open packing: unlike the alkali metals it undergoes no change of volume on entering into combination. Its atomic weight is 107·93; its density has been differently determined by different observers, for silver heated *in vacuo* Dumas found 10·512, and Roberts-Austen 10·57; we take the mean of these numbers 10·541. The atomic weight, 107·938, divided by 10·541, gives 10·233, the gross atomic volume. From this the diameter of the atomic sphere is found to be 2·17152, and its volume 5·3616.

Both the chloride and bromide crystallise in the cubic system, and are regarded from analogy as possessing the same crystalline structure and symmetry as the corresponding salts of potassium and sodium.

The specific gravity of silver chloride is obtained from the solid *after fusion*. The latest determinations give for silver chloride a

specific gravity of from 5.517 to 5.594, and for silver bromide from 6.215 to 6.425. Data for calculation are given in the table :—

	M. W.	Specific gravity.	Gross volume.	Diameter of atomic sphere or edge of crystal cubelet.
Ag.....	107.938	10.541	10.233	2.1715
AgCl.....	143.391	5.517	26.249	4.7175 (a)
		5.594	26.615	4.7395 (b)
AgBr.....	187.901	6.215	30.233	4.9452 (a)
		6.425	29.246	4.8907 (b)
AgI.....	234.798	5.675	41.374	

Salt, gross volume.	Silver, gross volume.	Difference of gross volume.
AgCl....26.249	10.213	16.036 chlorine (a)
26.615	"	16.402 " (b)
AgBr...29.246	"	19.033 bromine (a)
30.233	"	20.02 " (b)
AgI.....41.374	"	31.141 iodine.

We are now in a position to compare the diameter of atoms of silver in the free state and in combination. The diameter of chlorine being taken as 2.4954, the value found in Part I, and that of bromine 2.801, its value in sodium bromide, we obtain from Professor Miers' formula—

Diameter of Ag in—	
AgCl.	AgBr.
(a) 2.2279	(a) 2.1784
(b) 2.2399	(b) 2.1307

The results for (a) and (b) correspond to the different specific gravities (a) and (b) given above, and obtained by different observers.

The differences between the diameters (a) of silver in combination and that it possesses in the free state are not great.

On comparing the gross volumes of the haloids given above with their gross volumes in the haloid compounds of the alkalis, it will be observed that the gross volume of bromine in silver and sodium bromides is closely correspondent; this is because the atomic volumes of sodium and silver have nearly the same relative value.

Silver iodide, to which we now turn, is one of the most interesting substances we have yet encountered, and most important results follow from its study. At ordinary temperatures it does not present

itself in the cubic system, but in hemimorphic hexagonal crystals. For this reason we cannot, by the method we have previously employed, calculate the diameters of its component atoms, but a reference to the preceding table will show that the gross volume of the iodine is not greatly in excess of that which it possesses in sodium iodide, so that in all probability its atomic volume is the same, or sufficiently similar to serve as basis for argument. The diameter of the silver atom is then 2.1715, that of the iodine 3.1905. The ratio of the diameter of the silver to that of the iodine falls consequently below the limiting rates of 0.7286 : 1.0. The exact ratio both for this and the constituents of the other silver compounds is given in the following list:—

	Ratio of diameters of atoms.
Ag : Cl	= 0.8928 : 1
Ag : Br	= 0.7761 : 1
Ag : I	= 0.6894 : 1

From this it is evident that while the chloride and bromide are well within the 0.728 limit, the iodide lies outside it; and it follows from our hypothesis that while the two former salts may be, as they are, cubic, the iodide should not, but might be hexagonal hemimorphic, as it is.

When silver iodide is heated it contracts along the vertical axis (*c*) and expands in directions normal to this, till at a temperature of 146° C. it passes *per saltum* into the cubic system, and then with further rise of temperature expands uniformly. The transition from the cubic to the hexagonal system on cooling is accomplished almost with explosive violence, fragments of the solid iodide are projected into the air, and deep clefts extend into its substance. How on the basis of our reasoning is this curious transformation to be explained? Only on the supposition that the atomic volumes expand as the temperature of the substance rises; the mass, as a whole, may contract, as it certainly does, but this is to be accounted for by a change in the relative position of the atoms, the atomic volumes themselves must be conceived as all the time expanding. But it may be asked, is there any reason why the iodine and silver should expand at the same rate, and if not, which is the more likely to possess a higher coefficient of expansion, the iodine or the silver atom? The probabilities are all in favour of the iodine. The coefficient of expansion of silver in the free state is 0.000037 from 0° to the melting point, and 0.00002 from 0° to 100° C. That iodine has an immensely greater coefficient may be judged from the change in its specific gravity with rise of temperature, thus at 40.3° its sp. gr. is 4.917; at 60°, it is 4.886; at 79.6°, it is 4.857; at 107°, just before it liquefies, it is 4.825; after liquefaction, it is 4.004;

and at 151° , a point just above the temperature at which the hexagonal form of silver iodide is exchanged for the cubic, it is 3.866.

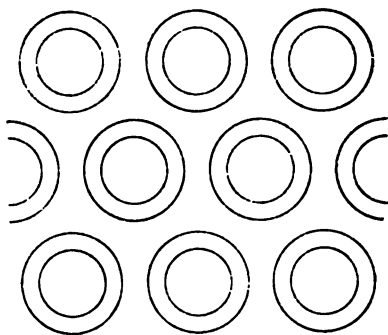
We may fairly therefore attribute the greater expansion to the iodine, the silver probably undergoing but slight increase in bulk. In what way does this help us? If the disparity between the volumes of the atoms were originally the cause which determined the silver iodide to assume hexagonal instead of cubic symmetry, how, by increasing this disparity, shall we render it cubic? Nothing can be simpler. By the time the substance has reached 146° C. the atomic volume of the iodine has become so great relatively to the silver that the latter is lost in the interstices, and it is the iodine alone which is directly operative in supporting the crystalline edifice. Here we have a basis for calculation. If the iodine thus forms the framework of the crystal, it must be, as we shall show later, because it is packed on that open system which we have already described and illustrated in the first part of this communication. From this the diameter of the iodine spheres can be calculated.

The volume of silver iodide, as deduced from Rodwell's data, is 40.67, just above 142° , the point which Rodwell gives as the critical temperature for the change of system. Supposing, as we have already stipulated, that the iodine is built up in most open cubic packing, we find from this volume the number 3.439 as the length of the diameter of the iodine atom.

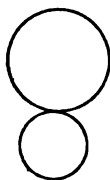
We shall make important use of this number directly, but we must first endeavour to find a probable structure for the silver iodide when crystallised in the hexagonal system.

It will conduce to clearness to avoid discussion at this stage, and to describe at once the structure which I am led to think is the only possible one consistent with the crystalline and physical properties of the compound. Let us regard the silver and iodine spheres as directly united together to form molecules of AgI ; conceive these placed on a horizontal plane with their major axes vertical, and disposed in triangular order as in the diagram (fig. 1). The balls must not be in contact laterally, but separated by definite regular intervals; over this first sheet place a second similarly formed, and so related to the first that the silver atoms rest in the alternate intervals left between the lower set of iodine balls taken in threes (fig. 2). The assemblage so produced will be homogeneous, hexagonal, and hemimorphic. It represents silver iodide as it exists at ordinary temperatures. Now, bearing in mind the fact that every silver atom rests in the interval between three iodine atoms, let the latter enlarge to a slight extent, and simultaneously retreat from each other along the horizontal plane to a small but uniform amount. This corresponds to the expansion along the lateral axes on heating. But as a result of this

FIG. 1.

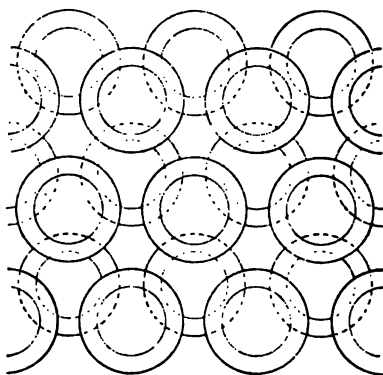


Plan of first sheet of molecules of AgI.



One molecule of AgI shown in elevation.

FIG. 2.



Plan showing two sheets of molecules of AgI.

expansion the atoms of silver will descend deeper and deeper between the atoms of iodine, and the distance between successive sheets in a vertical direction will diminish. This corresponds to contraction along the vertical axes with rise of temperature. On this hypothesis the anomalous contraction of silver iodide ceases to be an anomaly, but follows as a natural, though by no means necessary, consequence of its crystalline structure.

At the critical point, when the hexagonal is exchanged for cubic symmetry, the atoms of iodine possess, as already shown, a diameter of 3.439. Let them be represented in plan in the relative positions they must occupy for open cubical packing. They are shown in fig. 4 by a section taken at right angles to a trigonal axis.

Measured along the edge of the cube, the intervals between the atoms from centre to centre must, on this system of packing, be to the diameters precisely as $\sqrt{2} : 1$. Find the diameter of a small sphere that will just fit in between the three spheres as represented in the section. It is exactly 2.17687. But we have already found that the diameter of an atom of silver, both in the free state and in combination, measures at ordinary temperatures 2.1715, an extraordinary coincidence. The difference between this number and that just obtained, $2.1769 - 2.1715 = 0.0054$; again, the difference between the diameter of the atoms of iodine in the alkaline iodides at ordinary temperatures and the atoms of iodine in silver iodide at 142° is $3.439 - 3.2 = 0.239$, and these two numbers (0.0054 and 0.239) probably approximately represent the change in dimension of the constituent atoms of silver iodide as the temperature of this substance is raised through a range of 130° . Calculating from the coefficients given by Fizeau, the expansion of 0.0054, which we have found for the silver, is just twice that which it would experience in the free state.

Up to the critical point, when the interval between the iodine atoms has become large enough to allow the atoms of silver to pass between them, the atoms of silver have been supported by the atoms of iodine and have supported iodine in their turn; all the atoms have been subject to molecular pressure, but directly the critical position is attained, the atoms of silver are driven by this internal pressure into the middle of cubical clusters of atoms of iodine and partly or wholly released from pressure, the atoms of iodine alone then sustaining the crystalline fabric. It is not wonderful under these circumstances that the spectrum of silver iodide should differ markedly when observed below and above the critical point. Thus Wernicke has observed that at a high temperature the spectrum (of silver iodide), like that of solid and liquid iodine, contains no blue nor violet light. In the normal state, below 138°C. , silver iodide

gives a spectrum less bright, but twice as long and particularly developed in the blue and violet spectrum.*

The passage of silver iodide from one form to the other is, as has been stated, sudden and abrupt. This necessarily follows, from the geometrical conditions of the case. On reference to fig. 5 it will be seen by inspection that while a gradual descent of the atoms of silver may take place so long as their centres are situated above the centres of the atoms of iodine, against which they glide, yet directly after they come to lie in the same plane, a sudden descent must take place to a definite extent, which is given by the formula $DO_1 - DO_2 = r_1 + r_2 - 2r_1 \cos \theta$, or by the equivalent $\sqrt{(3r_1 r_2^2 - 4r_1^3)}$, where r_1 is the radius of the large spheres, r_2 of the small spheres, and $O_1 O_2$ the distance through which a sudden descent occurs.

The instantaneous descent of the silver which thus takes place is accompanied by a sudden change of volume in the compound itself; as determined by Rodwell, the volume diminishes from 1.01575 to 1.0, as the temperature passes above 142° , the maximum density of the salt then being attained. In the collocation of spheres which we have imagined, it is possible to compare the bulk before and after the critical point is passed very simply. A tetrahedron is constructed by joining the centres of three spheres of iodine below, which are in contact with one of silver lying on the axis above, with a fourth of iodine, also on the vertical axis and attached to the single sphere of silver. The ratio of the volume of this tetrahedron is directly proportionate to the whole volume of the structure, whether in the cubic or hexagonal systems, and at the critical point the volumes are directly proportionate to the heights of the respective tetrahedra. From this we find that the volume before contraction is to that after as 114 : 100, amply sufficient, and, it might be objected, superfluous; for the contraction, as observed by Rodwell, only amounted to from 1016 to 1000. This is a case, however, in which theory proves more correct than observation, for Mallard and Chatelier have shown† that Rodwell's results are erroneous, owing, as they remark, to his having deduced the cubical expansion from the linear extension, as though silver iodide were an isotropic body. These observers were able to bring about the transformation from the hexagonal to the cubic structure by the application of pressure (4,000 kilograms to the square centimetre), and they found that the ratio of the volumes before and after change, was as 116 to 100, which gives a coefficient ten times as great as that of Rodwell, and very closely in agreement with that (114) which we have theoretically

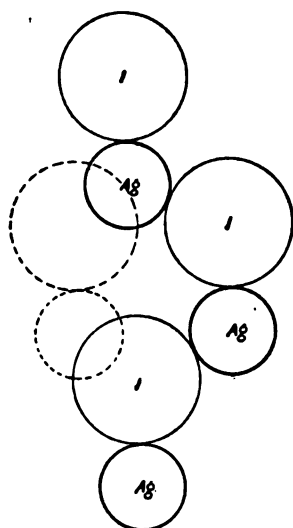
* Wernicke, 'Pogg. Ann.,' vol. 143, *vide* Rodwell, 'Proc. Roy. Soc.,' vol. 25, p. 206. Wernicke accounts for this by supposing some of the iodine to be liberated from combination.

† 'Bull. Soc. Min.,' vol. 7, 1884; 'Journ. Phys.,' vol. 4, p. 305, 1885.

deduced. Under the influence of rising temperature, the change was smaller, from 111 to 100, but this value is said to be merely approximate. That the change of volume should be smaller is not unintelligible; the direct effect of pressure is to bring about a change of configuration, which is unstable, since the substance reverts to its original state directly the pressure is relieved; the direct effect of heat, on the other hand, is to produce an expansion of the atomic volumes, and the change of configuration follows only as a consequence of this. In the difference between the value 111 and 116 we should have a measure of this increase in atomic volume, but for the probability that an expansion of atomic volume may take place, as a consequence of the change of configuration which results from the action of pressure. It is of interest to note that the amount by which the pressure must be reduced to reverse the operation and bring back the substance to the original state is only one-half of that required to produce the change directly; the reversal also takes place much more rapidly than the direct transformation.

It is to the beautiful observations of Fizeau that we owe our knowledge of the change in dimensions of silver iodide measured in relation to its crystalline axes. Along the axis c , Fizeau found a negative coefficient of expansion of -0.00000397 ; along two rectangular axes in a plane normal to c he found a positive coefficient of 0.00000065 . These were for a mean of 40° over an interval of from -10° to 70° ; the coefficient of variation $\Delta\alpha/\Delta\theta$ was found to be for c -4.27 , for the other axes 1.38 . Thus for the mean temperature the coefficient of contraction is six times that of the coefficient of expansion. A very significant relation, but not more so than the fact that the contraction increases as the temperature rises, and, as Rodwell's observations seem to prove, the increase becomes very considerable as the temperature rises above 70° . Every geometer will perceive at once that these relations are in absolute harmony with the conception we have framed of the ordering of the molecules in the crystal. They directly depend on the changing ratio of the sine and cosine of the angle θ as the small sphere (Ag) of the figure (fig. 6) is squeezed out from the three larger spheres (I) against which it is pressed (only one of these spheres (I) is shown in the figure). As the iodine atoms approach each other, with fall of temperature, the line aC will revolve round a as a centre, and thus the vertical parameter will expand as the horizontal contracts. From the coefficients given by Fizeau, it follows that at his mean temperature of 40° the angle θ is $9^\circ 18'$; from the crystallographic measurements of Zepharovich, made presumably at the ordinary temperature of the air, and therefore at or about 15° this angle appears to have become $22^\circ 17'$, as calculated for me by

FIG. 3.



Vertical section through one edge and centre of a face of a rhombohedron of three sheets.

FIG. 4.

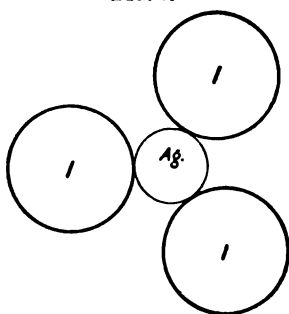


FIG. 5.

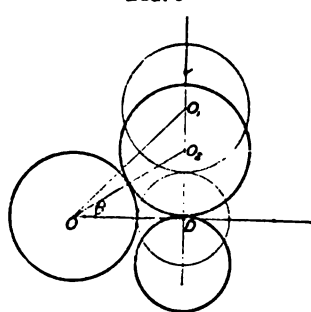
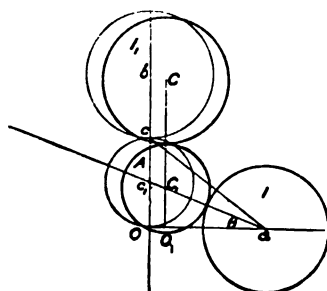


FIG. 6.



Professor Miers. The ratio of sine and cosine for this angle is $0.41 : 1.0$, which gives the relation that should subsist by hypothesis between the coefficient of expansion along the axes a and c at 15°C . Fizeau's observations fail us here, and I look forward with interest to fresh determinations of these constants, which Mr. Tutton has kindly promised to make for me. It is now possible to trace out the series of changes which silver iodide undergoes as it falls in temperature from 150° downwards; at 146° (Mallard and Chatelier) or 142° (Rodwell) it possesses cubic symmetry, because the atoms of iodine are out of all proportion so much larger than those of silver, that the latter cease to be operative as elements in the crystalline structure; the iodine atoms are arranged to form cubes which we may picture to ourselves as rhombohedra with an angle of 90° ; every iodine atom is chemically united with an atom of silver which depends from it into the central space within a rhombohedron; as the iodine atoms become smaller, with loss of energy, this space becomes too small to contain the silver atom which is consequently forced outwards or upwards; as it passes out of the rhombohedron of 90° the vertical parameter of the latter is necessarily lengthened, while its horizontal parameters are shortened; the change in dimension of the vertical parameter, which occurs as soon as the centre of the atom of silver begins to move above the plane containing the centres of three surrounding iodine atoms, is excessive as compared with that of the horizontal parameter, but the difference is diminished by the diminishing ratio between the diameters of the two different kinds of atoms; at 15° the difference of the two coefficients is probably nearly as $3 : 1$; it will diminish rapidly with further cooling, and, before the atoms of silver assume a position in which the radius vector drawn from them to the iodine atoms, about which they revolve, makes an angle of 45° with the parameter Oa , the expansion along the vertical axis will cease and be replaced by contraction, unless, indeed, as this crisis is approached the diameters of the atoms should fall within the ratio of $1 : 0.729$, when the crystal would once again become cubic.

In the study of molecular volumes we hold the key which is destined to unlock the secret of crystal structure so long concealed. We shall eventually discover by its means, in connection with other studies, the relative and absolute dimensions of all the elementary atoms, and probably not only this but also their true forms—for it is possible that they are not all spheres.

"On the Intimate Structure of Crystals. Part III. Crystals of the Cubic System with Cubic Cleavage." By W. J. SOLLAS, LL.D., D.Sc., F.R.S., Professor of Geology in the University of Oxford. Received March 8,—Read March 17, 1898.

The remaining metals and diatomic compounds which crystallise in the cubic system and possess cubic cleavage are few in number; some of them form the subject of the present communication. Triatomic compounds fulfilling these conditions are left for later consideration.

Ammonium chloride.—M. w., 53·506; sp. gr., 1·52 (Schröder); m. v., 35·201. Volume of four molecules, $35\cdot2 \times 4 = 140\cdot8$.

Edge of cubelet, or sum of the diameters of one molecule of NH_4 and one atom of Cl., $\frac{2}{3}140\cdot8$ 5·204

Diameter of one atom of Cl. 2·4954
 " " NH_4 2·7134

Gross volume of NH_4 , $2\cdot7134^3 = 19\cdot98$; volume of molecular sphere of NH_4 , $19\cdot98 \times \frac{4}{3}\pi = 10\cdot46$.

Galena, PbS .—M. w., 238·97; sp. gr., 7·25 to 7·77 (7·513 taken); m. v., 31·78; volume of four molecules, $31\cdot78 \times 4 = 127\cdot12$.

Edge of cubelet or diameter of $\text{Pb} + \text{S}$ 5·028

Diameter of one atom of Pb 2·625
 " " S 2·408

Lead enters into combination without change of volume. Gross atomic volume of Pb, $2\cdot625^3 = 18\cdot1$; volume of atomic sphere, $18\cdot1 \times \frac{4}{3}\pi = 9\cdot481$. Gross volume of S, $2\cdot408^3 = 13\cdot96$.

Lead selenide, PbSe .—M. w., 286·01; sp. gr., 8·154 (Little); m. v., 35·076. Volume of four molecules, $35\cdot076 \times 4 = 140\cdot305$.

Edge of cubelet or diameter of $\text{Pb} + \text{Se}$.. 5·1962

Diameter of one atom of Pb 2·625
 " " Se 2·571

Gross volume of Se, 17·0; volume of atomic sphere, 8·9.

Of the oxides which crystallise in the cubic system with cubic cleavage only three are sufficiently well known to afford data for

treatment; these are calcium, magnesium, and stannous oxides. The metals calcium and magnesium undergo a considerable amount of condensation on entering into combination; it will therefore be more convenient to select stannous oxide as the compound which is to serve as the basis for obtaining the relative diameter of oxygen; but this course is not without its disadvantages, for very different values have been found by different experimenters for the specific gravity of stannous oxide, and the same is true of the metal tin itself, which further has the additional defect of crystallising, not in the cubic, but in the tetragonal system.

Tin.—At. w., 118.1; sp. gr., 7.0 to 7.5; mean, 7.25 (taken).

Gross atomic volume, 16.29; volume of atomic sphere, 8.53.

Diameter of atom, 2.535.

Stannous Oxide, SnO .—M. w., 134.1; sp. gr., 6.1 (Nordenskjöld), 6.6 at 0°C . (Berzelius, Ditte). The latter value is taken.

M. v., 20.313. Volume of four molecules, 81.273.

Edge of cubelet or diameter of SnO	4.3316
--	--------

Diameter of one atom of Sn.....	2.535
---------------------------------	-------

" " O	1.8508
-------------------------------	--------

Gross volume of oxygen, 6.34; volume of atomic sphere, 3.32.

Calcium oxide, CaO .—M. w., 56; sp. gr., 3.251; m. v., 17.25.

Volume of four molecules, $17.25 \times 4 = 68.9$.

Edge of cubelet or diameter of CaO	4.100
---	-------

Diameter of one atom of O	1.851
---------------------------------	-------

" " Ca	2.27
--------------------------------	------

Gross volume of Ca, 11.695. The gross volume of Ca in the metallic state is 25.48.

Periclase, MgO .—M. w., 40.38; sp. gr., 3.636; m. v., 11.105.

Volume of four molecules, 44.423.

Edge of cubelet or diameter of MgO	3.5416
--	--------

Diameter of one atom of O	1.851
---------------------------------	-------

" " Mg	1.6936
--------------------------------	--------

Gross volume of Mg, 4.858. The gross volume of Mg in the metallic state is 14.341. It crystallises in the hexagonal system. These results are more or less uncertain; it must be borne in mind that the volume of oxygen differs greatly in different compounds.

The metals which remain for consideration in the present communication are as follows:—

Copper.—At. w., 63·8; sp. gr., 8·945; at. v., 7·0766. Volume of atomic sphere, 3·7053; diameter, 1·92.

Gold.—At. w., 197·2; sp. gr., 19·33 at 17·5°; at. v., 10·202. Diameter of atomic sphere, 2·169.

Iron.—At. w., 56; sp. gr., 7·85 at 16° (Caron); at. v., 7·134. Diameter of atomic sphere, 1·925; or sp. gr. 8·139 (Roberts Austen); at. v., 6·88; diameter of atomic sphere, 1·902.

Manganese.—At. w., 55; sp. gr., 7·3921 at 22°; at. v., 7·44. Diameter of atomic sphere, 1·952.

Platinum.—At. w., 194·8; sp. gr., 21·5 at 17·6°; at. v., 9·0605. Diameter of atomic sphere, 2·085.

Palladium.—At. w., 106; sp. gr., 11·4 at 22·5°; at. v., 9·2983. Diameter of atomic sphere, 2·103.

The Absorption of Hydrogen by Palladium.—Strong confirmatory evidence of the existence of the open packing which we have assigned to the metals crystallising in the cubic system is afforded by the phenomenon of solid solution (so called), and particularly by the absorption of hydrogen by palladium. When similar spheres are arranged in open cubic order, they form straight rows in contact, running parallel to the edges of the cube they constitute, and corresponding to these files of spheres are open galleries, lying between and running parallel with them. Through these galleries atoms, if small enough, might pass from end to end without encountering any obstacle, and thus the transpiration of hydrogen through metallic plates might be explained. Further, between every set of eight atoms, forming a primitive cube of the pile, the gallery widens out into a chamber, in which an atom smaller than that of the metal might conceivably lodge. The diameter of an atom which could occupy the space between eight atoms, forming a primitive cube, of palladium, can readily be calculated. The diameter of an atom of palladium has already been determined to be 2·103, the edge of a cubelet formed of eight atoms is therefore 4·206, and the length of the trigonal axis of such a cube is $4·206 \times \sqrt{3} = 7·285$; and $(7·285 - 4·206)/2 = 1·538$, the length of the diameter of an atom, which would just occupy the central space. This estimate, however, requires modification, by virtue of the fact that palladium progressively increases in volume as the absorption of hydrogen takes place. In Dewar's determinations the expansion was measured by the change produced in the specific gravity of the palladium; the lowest specific gravity which Dewar observed was 10·8033; this gives for the edge of the primitive cube a value of 4·2818. Assuming that the atoms of palladium have not increased in volume by absorbing energy, but have simply become more remote from one another, we may proceed as follows: $4·2818 \times \sqrt{3} = 7·416$, the length of the tri-

gonal axis of the primitive cube, and thus $(7.416 - 4.206)/2 = 1.605$, the true diameter of an atom which could just occupy the central interspace of the primitive cubelet. The cube of this number will give us the gross volume of the atom of occluded hydrogen; it is 4.134. If now we turn to Rücker's address on "The Range of Molecular Forces,"* we find the most probable estimates given for the volume of hydrogen (H) are as follows:—From K (Boltzman) 4.4, (Klemenčič) 4.4; from n (Mascart) 4.65; from b (Van der Waals and O. Meyer) 4.4. Between these numbers and that we have just obtained there is a very remarkable concordance.

It may further be observed that the number of such interspaces as we have considered is, to the number of atoms among which they lie, in the ratio of 1 : 1, so that from purely geometrical considerations it might be inferred that the limiting value for the absorption of hydrogen by palladium would be reached with the formation of the substance Pd_2H_2 . Observation shows that this limit is never exceeded, never even attained, while that which is reached may fairly be represented by the formula Pd_3H_2 . It is obvious that purely geometrical considerations are not all that are involved, and to discuss other factors would be to trespass beyond our province. There is one point in direct connection with our inquiry which must not, however, be disregarded. The value we have found for the diameter of hydrogen was obtained on the assumption that all the central spaces were occupied by hydrogen, which would only be the case if Pd_2H_2 were formed; the observed ratio, Pd_3H_2 , would lead us to believe that only two-thirds of the spaces are so occupied. This renders necessary a correction in our estimate, which would slightly increase the dimensions of the hydrogen atom. It is not possible, however, to introduce this correction, on account of the absence of information regarding the crystalline form assumed by Pd_2H_2 . If crystals of palladium be capable of taking a charge of hydrogen, there should be no difficulty in ascertaining whether a change in crystalline form accompanies occlusion. On the assumption that the maximum expansion of palladium due to occlusion is confined to two-thirds of the volume of the metal experimented upon, I find that the diameter of the hydrogen atom should be 4.395. Possibly the assumption is not defensible, but in any case it would appear that the amount of coincidence we have already obtained between the dimensions of the hydrogen atom, as calculated from the crystalline structure we have assigned to palladium (along with other metals) and the dimensions which follow from other modes of inquiry, affords strong confirmation of our hypothesis.

The absorption of hydrogen by potassium might easily take place without producing any marked expansion, i.e., so far as the relative

* 'Trans. Chem. Soc.,' vol. 63, p. 257, 1888.

dimensions of the atoms are concerned in the matter, for the inter-space in the centre of a primitive cube of potassium is large enough to house an atom of a gross volume exceeding 17.

In the case of iron the central space is notably smaller than in that of palladium; supposing no expansion to occur on absorption, the largest atom it could contain would have a diameter of 1.392, corresponding to a volume of 2.697.

It is probable, however, that a change in crystalline system is associated with the absorption of gases by iron and nickel. This is suggested by the curious effect produced on the nature of these metals by repeated absorption of hydrogen, at least in the case of nickel, which loses its cohesion and after repeated treatment becomes converted into a friable powder.

The galleries formed by ranges of central spaces present constrictions at intervals corresponding to the places where the four spheres forming the face of a cubelet are most closely approximate; the ratio of the diameter of a sphere that could just traverse one of these constrictions is to that of a sphere which would just occupy a central space as $\sqrt{2} : \sqrt{3}$. Hence the passage of an atom into the central chamber involves either a displacement of the atoms surrounding the entrance or a contraction in the volume of the entering guest. Is it possible that the "singing" of palladium, which accompanies the process of occlusion, is connected with vibratory movements of its atoms as they open and close the entrances to the central chambers?

In conclusion it may be pointed out that all the metals which are known to occlude hydrogen, viz., potassium, sodium, magnesium, iron, nickel, platinum, and palladium, are paramagnetic, sodium and magnesium being the only cases of an uncertain nature, while lead and gold, which offer roomy central spaces for the occupation of hydrogen, but do not absorb it, are diamagnetic.

"The Electrical Response of Nerve to a Single Stimulus investigated with the Capillary Electrometer. Preliminary Communication." By F. GOTCH, M.A., F.R.S., Professor of Physiology, University of Oxford, and G. J. BURCH, M.A. (Oxon). Received April 1,—Read May 12, 1898.

The electrical changes which are evoked in nerve by a single stimulus have up to the present been but little investigated. The examination of the phenomena has been almost entirely limited to observations upon the galvanometric deflections caused by the summed effects of a rapid succession of excitations, and rheotome methods,

carried out along these lines by Bernstein, Hermann and others, have yielded results of great value. It is, however, only on the assumption that the aggregate value of the successive electromotive states gives at any moment a faithful representation of each component member of the series, that deductions can be drawn from such rheotome observations as to the characters of the single electrical response. Attempts have been made to obtain indications of the response to a single stimulus by other methods, but without very satisfactory results. As far as we know, the only permanent record of such a response is that obtained by Gotch and Horsley in 1888 with the assistance of Burch; the record was that of the photographed excursion of a sensitive capillary electrometer.*

For some time the authors have been engaged in endeavouring to obtain with the capillary electrometer records of the single response of nerve which should be large enough not merely to indicate its occurrence but to afford data for determining its chief characteristics.

This object has been so far attained that they are now able to measure the electromotive changes of nerve in response to a single stimulus, by the application to the photographic records of the process of analysis introduced by one of them.† (G. J. B.)

The electrometer employed, made especially for the purpose by Burch, is more sensitive and, at the same time, more rapid in its action than any they have hitherto used. The latter quality, while essential to success, entails great liability to disturbance by mechanical vibrations, and considerable difficulty was met with on this account. The following form of support was ultimately adopted. A brick pillar was built up to the level of the ground upon a concrete foundation at the bottom of a pit 7 feet deep. On the pillar was placed a stout box containing some 5 cwt. of clay and upon the box three cast-iron plates, each weighing 1 cwt. Each plate was separated from the one below by three bags of sawdust, the bags forming supports, so arranged in opposite triangles as to come alternately under the nodes and loops of the plates. The electrometer, with its accompanying microscope, was fixed to the topmost plate, and was thus efficiently isolated from the rest of the apparatus and from the floor of the working room.

The excursions of the meniscus were recorded by a pendulum motor,‡ the image being projected by a Leitz 3 mm. objective upon the sensitized plate. This was carried by the motor across the optic axis in a circular arc at a distance of 125 cm. from the lens, giving

* 'Roy. Soc. Proc.,' vol. 45.

† 'Phil. Trans.,' A, vol. 183 (1892), pp. 81—105.

‡ See "The Capillary Electrometer in Theory and Practice," G. J. Burch; also Burdon Sanderson, 'Journ. Phys.,' vol. 18, pp. 126—134.

a magnification of 416 diameters. The velocity of the transit of the plates in the experiments now described varied between 14 and 70 cm. per second; it was determined in every instance by recording upon the plate the vibrations of a tuning fork having a period of 500 per second.

The sciatic nerves of large specimens of *R. temporaria* were used in all the experiments, and the present results were obtained during the winter months, i.e., from October to March.

The prepared nerve was placed in a moist chamber kept at from 4° to 6° C.; the chamber contained non-polarisable electrodes for the electrometer and polarising connections, and a pair of exciting electrodes. In every case the nerve was excited by a single stimulus applied to the sciatic plexus 20 to 30 mm. from the nearest of the electrometer contacts.

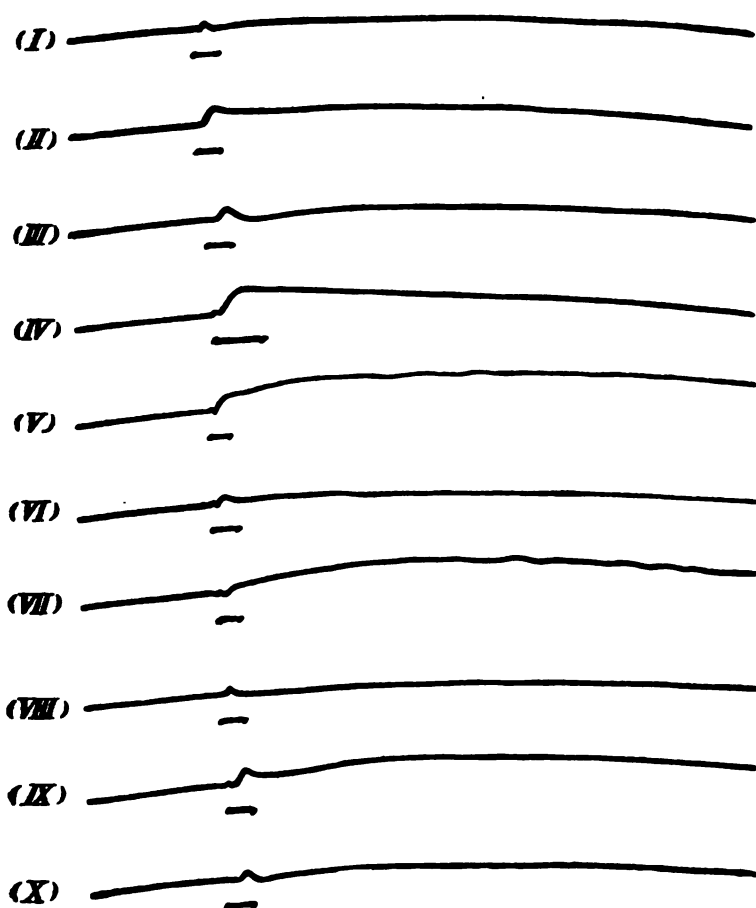
The form of stimulus usually employed was a relatively feeble induced current caused by the opening of the primary circuit of a standardised induction coil, which included one Daniell cell; the opening was effected by the pendulum motor and the primary coil contained no core. Each single stimulus of this type produces a movement of the meniscus when the electrometer contacts are suitably arranged upon the nerve. The movement is in some cases perceptible to the eye when the highly magnified image is projected upon a screen, but in many instances is only visible after the development of the photographic record. That the movement was not due to an escape from the exciting circuit is shown as follows:—The direction and character of the movement is unchanged whatever the direction of the exciting current; the escape, if present at all, is clearly indicated in the record as a rapid displacement of the meniscus, preceding by a distinct interval the larger movement which is here referred to; such antecedent escape excursions remain unmodified in character under conditions which materially affect both the larger movement and the physiological condition of the nerve (polarisation, CO₂, &c.); the escape, if present, is increased by augmenting the intensity of the exciting induced current, whilst the larger change reaches a maximum with a certain intensity of stimulus; finally, the larger movement is obtained by mechanical excitation such as the single tap of a light hammer arranged after the method of v. Uexküll.*

In order to facilitate the description of the excursions obtained, selected photographs have been projected upon a screen and the outlines of the variation in the level of the meniscus carefully traced; a number of different curves are thus brought into juxtaposition. A reduced copy of this is given in fig. 1. Some of the actual records will, it is hoped, be produced in a more extended communication, but the curves given in the figure are not merely faithful reproduc-

* v. Uexküll, 'Zeits. f. Biol.', vols. 31, 32, 1895.

tions of the originals, but correspond as regards dimensions with the records upon the photographic plates. The curves are to be read from left to right; the short line under each indicates $1/100$ of a second. In several instances, such as curves (i), (iv), (v), (vi), (vii), and (ix), the escape of the exciting current, arranged to be in the same direction as the change due to the response, is seen preceding this. When present it indicates the moment of excitation. In the actual records this moment was marked on each plate by photographing the movement of the opening key which caused the exciting break induction shock.

FIG. 1.



Freshly Prepared Nerve ; both Electrometer Contacts upon the Uninjured Surface.

The nerve in these preparations was dissected out from the spinal column to the knee, and the muscles below the knee were left attached to the preparation. With the electrometer contacts arranged at distances of 5 and 15 mm. respectively from the knee end, no resting difference of potential was perceptible. A single electrical or mechanical excitation of the sciatic plexus 45 mm. from the knee end of the nerve causes no visible displacement in the level of the mercurial meniscus, but the photographic record shows a rapid excursion indicating that the contact nearest the seat of excitation (proximal) is first negative and then positive to that more remote (distal). The character of the rapid up and down movement is indicated in curve (i) fig. 1. If the contacts are placed further apart (25 to 30 mm.) the spike present in the record is more pronounced, the ascending and descending limbs being further apart.

Freshly Prepared Nerve ; the Proximal Electrometer Contact upon the Uninjured Surface, the Distal upon a recently made Cross Section at the Knee End.

The contacts were arranged 10 mm. apart, and the well-known resting difference of potential existed between them. This demarcation difference caused a downward movement of the level of the meniscus amounting in some cases to 10 cm. upon the screen. It was not compensated by the use of any external E.M.F., but the pressure was altered so as to bring the meniscus up again to its proper position. During the presence of this permanent demarcation effect each single excitation of the plexus, whether electrical or mechanical, is followed by a visible displacement in the level of the image. The photographic record, see fig. 1 (ii), shows a rapid upward movement, followed by a prolonged tail in place of the downward movement obtained with uninjured nerve, thus indicating a more persistent change of the same sign as the initial one; this disappears comparatively slowly, and is of much lower E.M.F. than that which produces the initial rise.

Excised Nerve kept in the Cold from 24 to 90 hours in 0.6 per cent. NaCl made with Tap-water containing Traces of Calcium Salts.

That nerves kept in this way retain their excitability is shown by the fact that if the muscles are not detached they respond to excitation of the nerve trunks.

On connecting such an excised nerve with the electrometer, no

marked difference of potential is found to exist between the cross-section and any point on the surface.

A single excitation of the plexus rarely produces a displacement of the image of the meniscus which is visible to the eye, but in the photographic record such displacement is indicated by a definite excursion of the character shown in fig. 1 (iii). The form is a spike, the limbs of which are more widely separated than those of the uninjured fresh nerve fig. 1 (i). It indicates that the proximal electrometer contact becomes first negative and then positive to the distal one, but as the period at which the change of sign occurs is later than in the fresh nerve the rate of transmission of the excitatory process, of which the electrical change is an index, must be slower in the kept than in the fresh nerve.

If a new cross-section is made at the knee end and the distal contact placed upon it, the resting demarcation change is at once produced causing a marked downward displacement of the meniscus. Each single excitation of the plexus now causes a change plainly visible to the eye and the record shows as in fig. 1 (iv) that there is an excursion of considerable size, the initial rise being followed by the prolonged tail, which is seen in the freshly injured nerve.

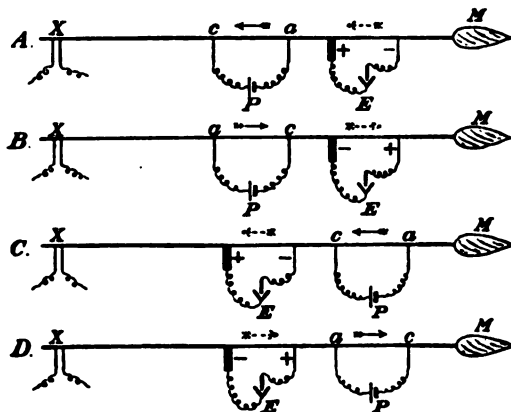
The Alterations produced by the passage of a Polarising Current.

It is well known that the passage of a galvanic current through a portion of nerve profoundly modifies its electromotive conditions. During the passage the regions outside the part traversed by the polarising current are the seat of changes such that currents of similar direction to the polarising one are present in the extrapolar regions.

It has been shown by Bernstein, Hermann and others that the excitatory effects are modified under these conditions, and a considerable part of the present investigation has been devoted to the determination of the character of the change evoked by a single stimulus under these circumstances.

The experiments were arranged along the lines shown in fig. 2. The excitation was restricted as before to the sciatic plexus, but an additional pair of non-polarisable contacts was connected with a circuit comprising a rheochord and two Daniell cells. These polarising contacts were placed either between the exciting and the electrometer ones as in arrangements A and B of fig. 2, or on the distal side of the electrometer ones as in arrangements C and D. In each case the polarising current may be reversed in direction and four different modifications can be thus studied. It will be observed that there is a fundamental difference as regards the position of the electrometer contacts, and this is accompanied by corresponding

FIG. 2.



X indicates the seat of excitation on the sciatic plexus; M the muscles below the knee; P indicates the circuit traversed by the polarising current; E the electrometer circuit. The unbroken arrow indicates the direction of the persistent polarising current in the nerve, the dotted one that of the persistent extrapolar effect thus produced. The anode and cathode of the polarising current are indicated by the letters *a* and *c* respectively. The signs (+) and (-) refer to the persistent difference of the electrometer contacts during polarisation.

differences in their electrical state. The proximal electrometer contact is that nearest the seat of excitation, and is represented in the figure by the broad dark line. In arrangement A it lies in the anodic extrapolar region, and owing to polarisation effects, is rendered positive to the distal one during the polarising flow; in B it is in the cathodic extrapolar region, and is negative to the distal one during such flow; in C it is in the cathodic region, but, as in A, is positive to the distal one, whilst in D it is in the anodic region, but is negative to the distal one as in the case of B.

The results as regards the character of the excitatory change differ in accordance with the particular arrangement employed, but it will be seen that with arrangements A and C, the records of the response to a single stimulus resemble one another inasmuch as the prolonged tail, previously referred to as characteristic of the nerve with a fresh cross-section, now becomes strikingly evident. On the other hand with B and D the records resemble that of the uninjured nerve, the spike alone being present.

It will be convenient to describe the results under two headings in accordance with this general subdivision; and, as experiments have been made on the preparations of all the types previously referred to, the results under each heading comprise those observed in fresh nerves uninjured and with cross-section, and in kept nerves.

Nerve during passage of a Polarising Current of such direction that the Proximal Electrometer Contact is positive to the Distal One.

(a) Fresh nerve prepared with electrometer contacts on surface and cross-section. The proximal contact is in this case positive to the distal owing to the resting difference; this is increased by the anodic extrapolar effect of a polarising current as in arrangement A (fig. 2). Each single stimulus of the plexus causes a very marked displacement of the meniscus plainly visible to the eye. The photographic record is of the character shown in fig. 1 (v). The initial rise is succeeded by a pronounced and prolonged effect of similar sign: it indicates that the prolonged change present in the unpolarised nerve and figured in fig. 1 (ii) is increased.

(b) Fresh nerve uninjured with both electrometer contacts upon the surface. There is no difference of potential between the contacts, but during polarisation the anodic extrapolar effect in arrangement A and the cathodic extrapolar effect in arrangement C are such that the proximal electrometer contact is positive to the distal. This is particularly the case with arrangement A owing to the nearer proximity of the contact to the polarised region.

The persistent extrapolar state is indicated by a downward movement of the meniscus which reaches a certain level and is raised by suitable alteration of pressure to the middle of the optical field. Each single stimulus applied to the plexus, although in the unpolarised nerve not followed by any displacement visible to the eye, produces now a visible change. The records show that the initial rise is still present, but that it is succeeded by a prolonged effect of similar character to that obtained in fresh nerve with the distal contact upon the cross-section. The curve given in fig. 1 (vii) shows its form.

(c) Nerve kept in 0.6 per cent. NaCl. It has been already stated that after twenty-four hours, the excised nerve gives no difference of potential when the electrometer contacts are placed upon cross-section and surface.

Polarisation may be produced of such character that the proximal electrometer contact (*i.e.*, that nearest the seat of excitation) is positive to the distal one, by arrangement A or C, as in the case of uninjured nerve. The effect caused by the single stimulus of the plexus is modified in the same way as in the previous class of observations. Instead of the spike which characterises the unpolarised nerve, the records show that the initial rise is followed by a prolonged effect of the form shown in fig. 1 (ix). As in the case of the uninjured nerve, this is more pronounced when the arrangement is that given in A than that of C (fig. 2) for the reason previously referred to.

If the kept nerve is subjected to a fresh cross-section, then the results of polarisation of this character resemble those described under (a) as occurring in the fresh nerve with a cross-section.

Nerve during passage of a Polarising Current of such direction that the Proximal Electrometer Contact is negative to the Distal One.

(a) Fresh nerve with electrometer contacts upon surface and cross-section. The arrangement made use of is that given in fig. 2, B, and the resting demarcation difference is now largely diminished or entirely overpowered by the polarisation extrapolar effect in the cathodic region. The single excitation of the plexus rarely produces any visible movement of the meniscus, and the record upon the plate is found to be of the character indicated in fig. 1 (vi). It is evident that this differs from both the unpolarised nerve, fig. 1 (ii), and the nerve in the state of opposite polarisation, fig. 1 (v), by the circumstance that the early part of the effect now takes the form of a small spike, and that the terminal prolonged portion forming the tail is reduced to very small proportions. The curve thus indicates that the proximal contact becomes first negative and then positive in rapid succession, whilst the prolonged effect no longer masks the second positive change as in the other instances just referred to.

(b) Fresh nerve uninjured, with contacts both upon the surface. Polarisation effects resulting in relative negativity of the contact proximal to the seat of excitation may be attained by either an arrangement like that given in fig. 2 B, or one like that of D. In the first case the electrometer contacts are in the cathodic extrapolar region; in the second case they lie in the anodic extrapolar region.

With either arrangement a single stimulus applied to the plexus produces no alteration in the level of the meniscus visible to the eye, but each plate when developed shows a record of the type indicated in fig. 1 (viii); the form of the photographed excursion is that of a very small spike the two limbs of which follow each other in rapid succession. The spike is sometimes, especially with arrangement D, followed by a curved prolongation dipping for a short distance below the resting level, but in no case has an effect in this direction been obtained at all comparable with that already referred to as producing the negative tail during the oppositely directed polarisation.

(c) Nerve kept in 0.6 per cent. NaCl. Polarisation producing relative negativity of the electrometer contact proximal to the seat of excitation can be attained by either of the two arrangements, B or D (fig. 2). When present, a single excitation causes no visible displacement of the meniscus, but the photographic records show that there is an abrupt up-and-down excursion of the type indicated in fig. 1 (x). When compared with that produced in the unpolarised nerve, fig. 1 (iii), the form of the spike is seen to be one in which the ascending and descending limbs are more closely approximated, and a slight dip below the resting level follows the completion of the excursion.

From the foregoing description it will be evident that when the proximal electrometer contact is relatively positive to the distal one, whether owing to the extrapolar changes due to the passage of a polarising current, or to the presence, in the case of a recent cross-section, of the resting demarcation difference, the electrical response evoked by a single stimulus has certain definite characteristics; the most conspicuous of these are the increased magnitude of the initial movement forming the ascending limb of the spike and the presence of a more prolonged change of similar sign to this. On the other hand when the contact proximal to the seat of the stimulus is relatively negative to the distal one and this latter therefore relatively positive, other characteristics of the response are accentuated; the initial movement forming the ascending limb of the spike is quickly checked and succeeded by one of opposite character, whilst the prolonged change disappears.

It would be beyond the scope of the present communication to enter upon any discussion as to the physiological meaning of the changes here referred to. It may, however, be pointed out that the results fully support the conclusions arrived at by Hermann from rheotome observations, viz., that those portions of the extrapolar region nearer the anode are during polarisation capable of a more pronounced excitatory electrical change than those more remote, whilst those portions of the cathodic region nearer the cathode are susceptible of a less pronounced change than those more remote.* New features are, however, brought to light by the present research as indicated in the preceding paragraphs.

The Electromotive Force of the Changes.

The records allow of the calculation of the E.M.F. of the potential difference between the contacts. The table appended to this communication gives five examples of the results of the method of analysis introduced by one of us† (G. J. B.) The five examples selected are the analyses of excursions such as are figured in curves (i), (ii), (iii), (vii), and (viii) of fig. 1. In addition to the intrinsic interest which attaches to the estimation of the E.M.F. of the electrical response to a single stimulus, the analyses present certain features which may be briefly alluded to.

The maximum E.M.F. it will be observed may reach as much as 0.032 volt (Table IV), a suggestive fact in relation to the view held by us as to the nervous origin of the E.M.F. of the response in the electrical organ of fishes.‡ This maximum is attained very rapidly

* See Hermann, 'Handbuch der Physiologie,' vol. 2 (i), p. 167.

† Burch, 'Phil. Trans.,' A, vol. 183 (1892), pp. 81—105.

‡ Gotch and Burch, 'Phil. Trans.,' B, vol. 187 (1896), p. 382.

310 *Electrical Response of Nerve investigated with Electrometer.*

Analysis of Five Records of Single Response.

The difference of potential between the proximal and distal contacts is given in terms of relative negativity (–) or positivity (+) of proximal as compared with distal contact. (Distance between contacts = 10 mm.) The differences of potential are given in decimals of a volt.

	I.	II.	III.	IV.	V.
Time after excitation of nerve 30 mm. from proximal contact.	Uninjured nerve. Electrometer contacts 10 mm. apart and both on uninjured surfaces.	Fresh nerve with cross-section. Proximal contact on uninjured surface + to distal on cross-section.	Nerve excised and kept 24 hours in 0.6 per cent. NaCl. Proximal contact on surface, distal on end.	Fresh uninjured nerve. Polarisation such that proximal contact is persistently + to distal one.	Fresh uninjured nerve. Polarisation such that proximal contact is persistently – to distal one.
sec.	start	start	nil	nil	nil
0.0020	start	start	nil	nil	start
0.0025	–0.0218	–0.0262	”	”	–0.0056
0.0030	–0.0218	–0.0176	”	”	–0.0081
0.0035	–0.0218	–0.0144	”	start	–0.0115
0.0040	–0.0002	–0.0127	”	–0.0083	–0.0115
0.0045	+0.0145	–0.0103	start	–0.0135	–0.0162
0.0050	+0.0128	–0.0081	–0.0093	–0.0182	–0.0220
0.0055	+0.0113	–0.0054	–0.0145	–0.0250	–0.0115
0.0060	+0.0101	–0.0029	–0.0178	–0.0329	–0.0001
0.0065	+0.0099	–0.0004	–0.0093	–0.0328	+0.0051
0.007	+0.0054	+0.0015	–0.0002	–0.0328	+0.0099
0.008	+0.0007	+0.0016	+0.0041	–0.0003	+0.0084
0.009	+0.0004	+0.0020	+0.0050	+0.0002	+0.0070
0.010	+0.0000	+0.0023	+0.0113	0.0000	+0.0058
0.011	..	+0.0023	+0.0116	0.0000	+0.0037
0.012	..	+0.0023	+0.0024	–0.0001	+0.0019
0.013	..	+0.0026	+0.0011	–0.0002	0.0000
0.014	..	+0.0029	+0.0010	–0.0005	
0.016	..	–0.0002	+0.0007	–0.0011	
0.018	..	–0.0019	+0.0003	–0.0016	
0.020	..	–0.0023	0.0000	–0.0022	
0.030	..	–0.0019	..	–0.0016	
0.040	..	–0.0013	..	–0.0010	
0.050	..	–0.0006	..	0.0000	
0.060	..	0.0000			

especially in the fresh nerve; the first indications of its presence in fresh nerve at 6° C. occur 0.002 second after the stimulation of the plexus when the seat of this stimulation is 30 mm. from the proximal electrometer contact. The response is thus propagated from the seat of stimulation at a rate of from 10 to 15 metres per second under these conditions. This is confirmed by the time relations of the culmination and commencing decline in the change, for in fresh nerve this begins in about 0.001 second with a distance of 10 mm. between the proximal and distal contacts. In the example of kept

nerve given in the third column, both the commencement and the culmination of the initial change are retarded, the propagation rate in this nerve at a temperature of 6° C. being slowed to 6 metres per second. A comparison of the fourth and fifth columns shows the retardation in the anodic as compared with the acceleration in the cathodic extrapolar region. Finally the time relations and the relative E.M.F. of the prolonged effect present in the instances given in the second and fourth columns may be compared with those of the initial change present in all the examples. It will be seen that the change producing the prolonged tail of the photographic record is one which differs from that producing the initial spike in the following important particulars: it develops slowly, taking from 0.006 to 0.01 second to culminate, its maximum E.M.F. is only one-tenth of that of the initial change, and it subsides slowly. It is not present in the instances given in columns I, III, and V.

In a more extended communication the authors hope to bring forward other features of the response of nerve, particularly the characters exhibited by the records of the changes produced by a series of stimuli and of those produced during reflex discharge of the central nervous system.

“On the Magnetic Susceptibility of Liquid Oxygen.” By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, London, &c. Received May 9,—Read May 12, 1898.

In a previous communication* we have described the initial investigations made by us to determine directly the numerical value of the magnetic permeability of liquid oxygen, and we there indicated that we hoped before long to present to the Royal Society the results of other experiments made by a different physical method which we anticipated would enable us to state whether liquid oxygen has a constant magnetic susceptibility, or whether, like a ferromagnetic substance, its magnetic susceptibility alters when subjected to different magnetic forces.

We have recently obtained results which, though limited to a certain range of force, we think afford fairly trustworthy values of the magnetic susceptibility of liquid oxygen under magnetising forces varying from 500 to 2500 C.G.S. units, and place them therefore on record.

* ‘Roy. Soc. Proc.’ 1896, vol. 60, p. 288, “On the Magnetic Permeability of Liquid Oxygen and Liquid Air.”

The method used by us in these last experiments depends on the well-known fact that if a body, either paramagnetic or diamagnetic, is placed in a magnetic field of variable strength, it is subjected to a mechanical force tending to displace it in the direction in which the field varies most rapidly.

If the susceptibility of the body is so small that it does not sensibly disturb the distribution of the field, the measurement of this mechanical force may be made to furnish a knowledge of the absolute value of the magnetic susceptibility.

The necessary conditions are, however, that the volume (V) of the body must be of such small magnitude relatively to the form of the field that its magnetisation is not sensibly different from that which it would obtain if immersed in a uniform field, and also that the magnetic susceptibility (k) of the substance must be of small absolute value. Under these circumstances, if f is the mechanical force (reckoned in dynes) acting on the body, and H is the strength of the field at its centre, then the force in the direction x is given by the equation

$$f = kVH \frac{dH}{dx}.$$

The value k thus determined is a difference value, that is, it is equal to the difference between the susceptibility of the body and that of the medium in which it is immersed. Hence if one and the same body is placed in the same divergent field, but alternately surrounded by different media, the difference in the apparent susceptibilities of the body in the two cases will give us the difference of the true susceptibilities of the media. The experimental method employed by us consisted, therefore, in determining the forces acting on a small sphere of known susceptibility when suspended first in air, and next in liquid oxygen, and hence deducing the difference of the susceptibility of liquid oxygen and air, and therefore the absolute value of the susceptibility of liquid oxygen, knowing that of air.

The first step was the construction of an electro-magnet capable of producing the required field. From the above-named conditions of success it will be seen that since the volume and susceptibility to be measured are both small, it is essential that the magnetic field shall not only be large but must vary very rapidly, or else the forces to be measured will be small.

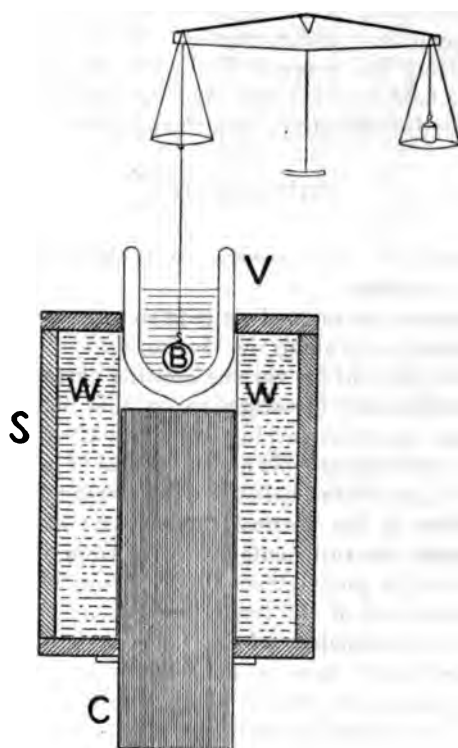
An electro-magnet giving the required field was therefore designed as follows:—

The exciting bobbin consists of a single coil of double cotton-covered copper wire, No. 14 S.W.G. in size.

The coil is 30 cm. long, 18.5 cm. in outside diameter, and the aperture in the coil 9.5 cm. in diameter. The total weight of

double cotton-covered copper wire on the bobbin is 71 lbs., and the total length of wire is roughly about 3300 feet. The total number of turns of wire on the bobbin is 2478. The wire is capable of carrying 12 or 14 amperes for considerable periods of time without dangerous heating. The total resistance of the wire is very nearly 5.5 ohms at 20° C.

This bobbin of wire is enclosed in a cylinder of mild steel of the same height as the bobbin, the walls of which are 2.5 cm. thick. This cylinder has movable circular end plates of steel 2.5 cm. in thickness fitting it, and in these plates are circular holes 9 cm. in diameter. A soft steel core was also provided,



V, vacuum vessel ; B, ball weighed ; W, wire windings of magnet ; S, C, steel shell and magnet core.

37 cm. in length and 9 cm. in diameter, and having steel studs screwed into it so that the core could be held in the position shown in the diagram. The end surface of the inner core was 7.5 cm. below the upper surface of the wire coil. The steel used is high permeability magnet steel.

On passing a current through the magnet coils a magnetic field is created in the space just above the upper end of the inner steel core, which is very divergent, and which is not only strong, but varies very rapidly in an axial direction.

This magnet was placed underneath a shelf on which stood a very sensitive short-beam chemical balance capable of weighing to one-tenth of a milligram. From one pan depended a fine wire passing through a hole in the balance case and shelf, and served to suspend a ball just within the field of the magnet.

If the ball was counterpoised by weights and the magnet then excited, the ball was subjected to an upward or downward force which either decreased or increased its apparent weight, according as it was diamagnetic or paramagnetic. If W is the gain or loss in weight on exciting the magnet, V the volume of the ball, H the strength of the field at its centre, and dH/dx the rate of change of the field in a vertical direction, then the equation

$$981W = k_1 V H \frac{dH}{dx}$$

gives us the value of the apparent magnetic susceptibility (k_1) in air of the body weighed.

In order to apply the method we require to know the value of the field H at different parts along the vertical axis of the magnet and also the value of the field for various exciting currents.

A careful preliminary investigation on this point was therefore made. Constant currents varying from 1 to 12 or 14 amperes were passed through the magnet coils and by means of a small secondary coil attached to a calibrated ballistic galvanometer the field strength at various points in the vertical central axis of the magnet was measured. These measurements extended from close contact with the magnet core to a point about 10 cm. above the end surface of the recessed polar end of the inner steel core.

The distance in centimetres of any point from the end polar surface of the inner steel core is denoted by x , this measurement being made as exactly as possible along the central vertical axis of the magnet. The strength of the field at this point in C.G.S. units is denoted by H , and the space rate of change in the x direction by dH/dx .

The following tables give the exciting currents (A) in amperes measured by a Weston ammeter, No. 3134, and the values of x and H .

The value of dH/dx at any point can be at once determined from the curve of H in terms of x .

Table I.—Magnetisation Curves of Tubular Electromagnet, the Induction Density or Field being measured at a point x centimetres from the Pole Face on the Axial Line.

Exciting current by Weston ammeter, No. 3134.	True current in amperes. A.	Induction density (H) at various distances x from the pole.		
		$x = 0.15$ cm.	$x = 2.70$ cm.	$x = 5.25$ cm.
15	15.15	4374	—	—
14	14.141	4200	—	—
13.9	14.040	—	3269	—
13	13.127	4054	3194	—
12	12.094	3848	3070	2002.5
11	11.124	3648	2888	1897
10	10.075	3407	2712	1782.5
9	9.082	3194	2543	1682
8	8.059	3014	2310	1557
7	7.059	2764	2153	1418.5
6	6.020	2504	1957	1273
5	5.015	2220	1740	1139
4	3.968	1884	1459	955.5
3	2.994	1481	1161	749
2	1.958	992	767	491.5

The induction density or field along the axial line was also measured for two constant excitations corresponding to currents of 12.094 and 6.020 amperes at various distances along the axial line, and the results are given in Table II.

The results in Tables I and II were then set out carefully in a curve, and from these curves an interpolation table constructed, showing the axial field at various points on the axis for different exciting currents, and these values are given in Table III.

Having calibrated the magnet, the following objects were provided to be used as bodies to weigh in the field, viz.:—

(1) A silver ball, (2) a rather smaller copper ball, (3) several hollow glass balls containing a little mercury, and (4) a bismuth ball.

In order to test the method and obtain confidence in the results, we made a number of preliminary measurements on the magnetic susceptibility of water, solutions of manganese sulphate, and ferrous sulphate of known densities.

These experiments were made by weighing the silver ball or hollow glass ball when immersed in these liquids contained in a beaker placed in the field cavity of the magnet.

Five weighings were always made—

- (1) The weight of the ball in air, magnet not excited.
- (2) The apparent weight of the ball in air, magnet excited.

- (3) The apparent weight of the ball in water, magnet not excited.
- (4) The apparent weight of the ball in the solution used, magnet not excited.
- (5) The apparent weight of the ball in the liquid, the magnet being excited with a known current, and the position of the ball in the field accurately known.

From these weighings we obtain all the required information. For from (1) and (3) we obtain the volume of the ball, and from (1), (2), and (3) the magnetic susceptibility of the ball, and from (4) and (5) the magnetic susceptibility of the liquid.

The sphere used as a testing substance was suspended by a long fine platinum or gold wire from one pan of the balance. A small beaker filled with water or the salts solution under test was placed over the pole of this magnet and the sphere suspended in it.

In some cases when using strong exciting currents we found it

Table II.—Induction Densities (H) of Field of Tubular Electro-magnet at various distances x along the Axial Line.

Distance from pole face = x cm.	Field = H in C.G.S. units.	
	For 6·020 amperes.	For 12·094 amperes.
0·15	2485·5	3853
0·58	2475·5	3825
1·01	2404	3711
1·44	2307·5	3582
1·87	2196·5	3419
2·30	2068	3225
2·73	1935	3016
3·16	1794	2830
3·59	1666	2624
4·02	1557	2453
4·45	1455·5	2291
4·88	1346	2128
5·31	1253	1960
5·74	1151·5	1820
6·17	1052	1666
6·60	965·5	1521
7·03	867	1385
7·46	770	1260
7·89	680	1099
8·32	596	955
8·75	509·5	805
9·10	425	681
9·61	356·5	564
10·04	294·5	462
10·47	231·4	368
10·91	189·9	289
11·33	142·1	228
11·76	114·2	175

Table III.—Summary of Results of Field Measurements of Axial Field of Tubular Electromagnet.

Current by Weston ammeter, No. 3134.	Distance from pole face on axial line.			Ratios of induction density or field H to field corresponding to 6·0 or 12·0 true amperes.		
	0·15 cm.	2·70 cm.	5·25 cm.	0·15 cm.	2·70 cm.	5·25 cm.
0	0	0	0	0	0	0
2	994	762	492	0·3989	0·3908	0·3862
3	1476	1148	742	0·5923	0·5887	0·5825
4	1778	1460	952	0·7135	0·7487	0·7473
5	2220	1726	1128	0·8908	0·8852	0·8854
6	2492	1950	1274	1·0000	1·0000	1·0000
7	2760	2158	1422	1·1075	1·1066	1·1161
8	2996	2346	1556	1·2022	1·2031	1·2218
9	3226	2534	1678	1·2945	1·2995	1·3171
9	3226	2534	1678	0·8402	0·8363	0·8466
10	3438	2712	1782	0·8954	0·8950	0·8991
11	3650	2880	1890	0·9506	0·9504	0·9536
12	3840	3030	1982	1·0000	1·0000	1·0000
13	4028	3178	2080	1·0490	1·0488	1·0494
14	4208	3320	2168	1·0959	1·0957	1·0939
14·5	4292	3390	2210	1·1177	1·1188	1·1153
15	4374	3460	2254	1·1391	1·1420	1·1372

necessary to pack round the beaker of liquid with crushed ice in order to keep the temperature of the water or solution from being raised by radiation of heat from the magnet coils. Any rise of temperature in the liquid under test, by causing variation in density, introduces a difficulty in obtaining exact and comparable weighings of the ball.

The position of the ball in the magnetic field was very carefully adjusted, so that the centre of the ball was as nearly as possible on the axial line of the magnet. The distance of the centre of the ball from the end of the magnet core was also measured with as much accuracy as possible when the balance was lifted off its supports, and yet the balance index was in the zero position.

The distance from the pole face measured in centimetres is called the *distance from the pole* in the following tables. Corresponding to this distance (x) we know from the preliminary experiments and the results given in Tables I, II, and III the strength of the magnetic field (H) and its axial rate of variation dH/dx .

The dimensions and weights of the balls used were as follows:—

Ball made of	Weight in grams.	Diameter in cm.	Density.	Volume in c.c. at 15° C.	Volume in c.c. at -182° C.	Mean coefficient of cubical expansion between 0° C. and -182° C.
Silver	132·010	2·90	10·334	12·775	12·654	0·0000518
Copper	37·610	2·00	9·92	4·226	4·190	0·0000427
Bismuth	62·220	2·27	9·836	6·326	6·287	0·0000320
Glass ball containing mercury, No. 1..	18·663	2·32	—	6·539	6·513	0·0000200
Glass ball containing mercury, No. 2..	3·400	1·40	—	1·462	1·456	„
Glass ball containing mercury, No. 3..	32·500	2·68	—	10·343	10·340	„
Glass ball containing mercury, No. 4..	5·95	1·03	—	0·588	0·586	„

In each case a measurement was made of the magnetic susceptibility in air of the ball used, by weighing it in a known magnetic field and observing the loss or gain in weight.

In this manner the following determinations were made:—

The Magnetic Susceptibility of the Silver Ball.

Weight of silver ball in air at 15° C. = 132·010 grams.

Weight of silver ball in water at 2° C. = 119·235 grams.

Volume of silver ball at 15° C. = 12·775 c.c.

	I.	II.	III.	IV.
Loss in weight in grams of silver ball when weighed in field = W	0·0287	0·0284	0·0142	0·0132
Exciting currents of magnet in amperes	12	12	6	6
Magnetic field in C.G.S. units = H	3320	2820	1798	2206
Field variation = dH/dx	426	459	308	258
Distance of centre of ball from pole in cm.	1·96	3·18	3·18	1·83
Calculated apparent susceptibility = $k \cdot 10^{-6}$	1·56	1·69	1·97	1·78

The absolute magnetic susceptibility of air is $0·024 \times 10^{-6}$.

The mean apparent susceptibility of silver in air is therefore from

the above figures equal to -1.75×10^{-6} , and the absolute value $k = -1.73 \times 10^{-6}$.

E. Becquerel gives -1.74×10^{-6} as his observed value for silver.

The Magnetic Susceptibility of the Bismuth Ball.

Weight of bismuth ball in air at 15° C. = 62.220 grams.

Volume of bismuth ball at 15° C. = 6.326 c.c.

	I.	II.	III.
Loss in weight in grams of bismuth ball when weighed in field = w ...	0.085	0.067	0.030
Exciting current of magnet in amperes	14.5	12	6
Magnetic field	2383	2135	1355
Field variation	415	372	236
Distance of centre of ball from pole = x in cm.	4.86	4.86	4.86
Calculated apparent susceptibility = $k \cdot 10^{-6}$	-13.4	-13.3	-14.6

The absolute magnetic susceptibility of air is 0.024×10^{-6} . The mean apparent susceptibility of bismuth in air is, therefore, from the above figures equal to -13.77×10^{-6} , and the absolute susceptibility $k = -13.75 \times 10^{-6}$.

Recent values found by other observers are -13.4×10^{-6} (P. Curie), and -13.3×10^{-6} (L. Lombardi). Hence our own is in accordance with these.

The Magnetic Susceptibility of the Copper Ball.

Weight of copper ball in air at 15° C. = 37.610 grams.

Volume of copper ball in air at 15° C. = 4.226 c.c.

	I.	II.	III.	IV.
Gain in weight in grams of copper ball in field	0.009	0.005	0.0055	0.0035
Exciting currents of magnet in amperes	6	3	6	3
Magnetic field	1220	709	518	298
Field variation	227	132	193	111
Distance of centre of ball from pole in cm.	5.46	5.46	8.72	8.72
Calculated apparent susceptibility = $k \cdot 10^{-6}$	+7.6	+12.5	+12.9	+28

The copper ball proved to be ferromagnetic, owing, no doubt, to traces of iron. We constructed a curve by which its susceptibility in various fields could be deduced from the above observations, but the only reason we employed it was because it seemed desirable to determine the absolute susceptibility of the liquid oxygen with bodies as far as possible different in susceptibility. Hence we selected silver, bismuth, and the above slightly ferromagnetic copper ball for the purpose.

The Magnetic Susceptibility of various Glass Balls partly filled with Mercury.

	No. 1 ball.	No. 2 ball.	No. 3 ball.	No. 4 ball (nearly full of mercury).
Gain or loss in weight in grams of ball when weighed in field	-0.001	{ This ball proved to be quite neutral in the strongest field we could produce }	-0.0044	-0.0007
Exciting current of magnet in amperes	6		12	12
Magnetic field	1330	—	3180	2418
Field variation ...	231	—	474	391
Distance of centre of ball from pole of magnet in cm.	4.96	—	2.38	4.115
Calculated apparent susceptibility = $k \cdot 10^{-6}$	-0.5	zero	-0.277	-1.23

We have, then, the following data for the glass balls:—

	No. 1 ball.	No. 2 ball.	No. 3 ball.	No. 4 ball.
Weight in grams...	18.663	3.400	32.500	5.95
Volume in c.c. at 15° C.	6.539	1.462	10.3427	0.5882
Diameter in cm. ...	2.32	1.40	2.63	1.03
Absolute magnetic susceptibility in vacuum	-0.48×10^{-6}	-0.024×10^{-6}	-0.25×10^{-6}	-1.21×10^{-6}

As a further check on the method we employed the above determinations of the susceptibilities of the silver and glass ball, No. 3, to obtain the value of the susceptibility of distilled water. The measurements were as follows:—

Magnetic Susceptibility of Water.

- I, with silver ball { susceptibility of ball = -1.73×10^{-6} .
volume of ball = 12.775 c.c.
II, with glass ball, No. 3 { susceptibility of ball = -0.25×10^{-6} .
volume of ball = 10.3427 c.c.

	I.	I.	II.
Gain or loss in weight of ball when weighed in field and in water	-0.0146 gram loss	-0.0163 loss	+0.0053 increase
Exciting currents of magnet in amperes	12	14.5	12
Magnetic field	2802	2848	3180
Field variation.....	456	465	474
Distance of centre of ball from pole of magnet in cm.....	3.22	3.79	2.88
Apparent susceptibility of ball in water = $k \cdot 10^{-6}$	-0.878	-0.945	+0.333

The absolute susceptibility of water is, therefore,

$$-(1.73 - 0.88)10^{-6} = -0.85 \times 10^{-6}$$

and $-(1.73 - 0.94)10^{-6} = -0.79 \times 10^{-6}$ from the experiments with the silver ball; and $-(0.333 + 0.253)10^{-6} = 0.59 \times 10^{-6}$ from the experiments with the glass ball No. 3. The mean of these values gives -0.74×10^{-6} as the absolute susceptibility of water. The following are some of the values obtained for the magnetic susceptibility of water by older and by more recent observers.

Observer.	Value of $k \cdot 10^{-6}$ for water.
Faraday	0.72
E. Becquerel	0.67
P. Curie	0.79
Townsend.....	0.77
Quincke.....	0.81
Du Bois	0.84

Mean value = 0.77

Hence our value for water $0.74 = k \cdot 10^{-6}$ is not far from the mean of the above results.

Many other experiments were then made with various solutions of salts of iron and manganese, which satisfied us that we could place reliance upon the results of this method in measuring the magnetic susceptibility of a liquid, and we then proceeded to experiments with liquid oxygen.

The balls were accordingly all weighed in liquid oxygen contained in a vacuum vessel placed over the pole piece of the magnet. This vacuum vessel contained mercury in its vacuum space and was of an unusually excellent kind. In it liquid oxygen could be preserved for periods of many hours without a trace of ebullition, and no difficulty was experienced in making the weighings with great accuracy. These weighings of course served also to determine the density of the liquid oxygen used. The results are embodied in the following tables.

In each case the weighings give the apparent susceptibility of the liquid oxygen, and these figures have to be corrected by adding or subtracting a number representing the absolute susceptibility of the ball at the liquid oxygen temperature. Thus in the case of the silver ball the figure subtracted is two, as the nearest integer representing the susceptibility of silver at -182°C . In the case of the bismuth ball the figure subtracted is sixteen, in the case of the glass balls it is zero or at most unity, and in the case of the copper ball the correction is additive, depending on the value of the field.

Table V.—Determinations of the Magnetic Susceptibility of Liquid Oxygen.

I. With the Silver Ball.

Volume of ball = 12.684 c.c. at temperature of liquid oxygen.

Density of liquid oxygen = 1.1376.

Distance of centre of ball from pole of magnet = 5.37 cm.

Magnetic susceptibility of silver ball = 1.73×10^{-6} .

Exciting current by Weston ammeter, No. 3134.	Field. H.	Field variation, dH/dx .	Loss in weight of ball in grams., W.	Apparent susceptibility, $k_1 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k 10^{-6}$.
15	2218	408	3.7585	322	320
14.5	2174	400	3.6242	323	321
12	1950	359	2.8845	319	317
9	1646	302.5	2.0365	317	315
6	1242	228	1.2303	337	335
4	928	170.4	0.6855	336	334
3	723	132.7	0.4243	343	341

Mean value of susceptibility of liquid oxygen as determined with the silver ball = 326×10^{-6} .

II. *With the Bismuth Ball.*

Volume of ball at temperature of liquid oxygen = 6.287 c.c.

Density of liquid oxygen = 1.1397.

Distance of centre of ball from pole = 4.865 cm.

Magnetic susceptibility of bismuth ball = 13.75×10^{-6} at 15° C.

" " " = 15.9×10^{-6} at -182° C.

Exciting current by Weston ammeter.	Field.	Field variation.	Loss in weight of ball in grams.	Susceptibility, $k, 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k, 10^{-6}$.
12	2135	373	1.655	325	309
6	1358	236	0.705	344	328

Mean value of susceptibility of liquid oxygen as determined with the bismuth ball = 319×10^{-6} .

III. *With the Copper Ball.*

Volume of ball at temperature of liquid oxygen = 4.190 c.c.

Density of liquid oxygen = 1.140.

Distance of centre of ball from pole = 5.46 cm.

Magnetic susceptibility (varies from 8×10^{-6} to 15×10^{-6}).

Exciting current by Weston ammeter.	Field.	Field variation.	Loss in weight of ball in grams.	Apparent susceptibility, $k, 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k, 10^{-6}$.
6	1220	227	0.3721	315	323
5	1080	201	0.2930	316	325
4	911	169.5	0.2096	318	329
3	709	131.9	0.1282	321	333
2	470	87.4	0.0546	311	326

Mean value of susceptibility of liquid oxygen as determined with the copper ball = 327×10^{-6} .

IV. *With the Glass Ball No. 1.*

Volume of ball at temperature of liquid oxygen = 6.513 c.c.

Density of liquid oxygen = 1.1391.

Distance of centre of ball from pole = 4.96 cm.

Magnetic susceptibility of glass ball = 0.48×10^{-6} .

Exciting current by Weston ammeter.	Field.	Field variation.	Loss in weight of ball in grams.	Apparent susceptibility, $k_1 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k \cdot 10^{-6}$.
7	1480	257	0.815	323	322.5
6	1330	231	0.671	329	328.5
3	777	134.9	0.229	329	328.5

Mean value of susceptibility of liquid oxygen as determined with the glass ball No. 1 = 326×10^{-6} .

V. *With the Glass Ball No. 2.*

Volume of ball at temperature of liquid oxygen = 1.456 c.c.

Magnetic susceptibility of ball = 0.024×10^{-6} .

Distance of ball from pole.	Exciting current of magnet.	Field.	Field variation.	Loss in weight of ball in grams.	Apparent susceptibility, $k_1 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k \cdot 10^{-6}$.
4.57	14.5	2500	427.5	0.4919	310.1	310
	14.5	2500	427.5	0.4916	309.9	310
	13	2350	402	0.4333	309.1	309
	12	2240	383	0.3936	309.1	309
	12	2240	383	0.3947	310.0	310
	6	1422	236.5	0.1721	344.8	345
	6	1422	236.5	0.1723	345.2	345
	12	1379	317	0.2260	348.4	348
7.04	12	1379	317	0.2252	347.1	347
	6	865	215	0.0894	323.9	324
	6	865	215	0.0894	623.9	324

Mean value of susceptibility of liquid oxygen as determined with the glass ball No. 2 = 325×10^{-6} .

VI. With the Glass Ball No. 4.

Volume of ball at temperature of liquid oxygen = 0.5858 c.c.

Magnetic susceptibility of ball = -1.23×10^{-6} .

Distance of ball from pole.	Exciting current of magnet.	Field.	Field variation.	Loss in weight of ball in grams.	Apparent susceptibility, $k, 10^{-6}$.	Absolute susceptibility of liquid oxygen, $k, 10^{-6}$.
4.935	15	2392	437	0.1912	306	305
	15	2392	437	0.1902	305	304
	12	2102	384	0.1475	306	305
	6	1337	234	0.0628	336	335
	12	2102	384	0.1477	306.5	305
	6	1337	234	0.0620	332	331
4.115	12	2418	391	0.1795	318	317
	6	1532	238	0.0790	361	360

Mean value of susceptibility of liquid oxygen as determined with glass ball No. 4 = 320×10^{-6} .

The absolute susceptibility is derived from the apparent susceptibility by adding or subtracting the susceptibility of the ball used according as it is paramagnetic or diamagnetic.

In the case of the copper ball, owing probably to traces of iron, the paramagnetic susceptibility is rather large and varies with the field. The proper additive correction was obtained by drawing a curve and setting off the observed values of the copper ball susceptibility as ordinates corresponding to the proper field strengths.

The mean value from all the six sets of observations comprising thirty-six determinations with the silver, copper, bismuth, and four glass balls is to give a value of 324×10^{-6} as the mean co-efficient of magnetic susceptibility of liquid oxygen. From the relation $\mu = 1 + 4\pi k$, the magnetic permeability can be deduced, and if $k = 324 \times 10^{-6}$ we have $\mu = 1.0041$, as the value of the permeability of liquid oxygen.

The value of μ we gave (see 'Proc. Roy. Soc.,' vol. 60, p. 292) as the result of our former experiments by a totally different method was $\mu = 1.00287$, or nearly, 1.003.

Hence these two methods agree in giving values of the magnetic permeability of liquid oxygen differing only by about one part in a thousand.

The results of the present work must, however, be taken as giving a much more probable value of the magnetic susceptibility.

On examining the above results it will be seen that there is a

general tendency for the susceptibility with large fields of the order of 2500 to be less than the susceptibility for fields of the order of 500. The average susceptibility in fields of from 2500 to 1900 is more nearly 310×10^{-6} , and that in fields from 1100 to 500 more nearly 330×10^{-6} .

The difference only amounts to about 10 per cent. of the lower value, and it cannot be said that the observations are all of exactly equal weight.

The general result is to show that between the limits of $H = 500$ and $H = 2500$ the average magnetic susceptibility of liquid oxygen has a value which does not differ much from 324×10^{-6} , but with a small but decided tendency to decrease in strong fields.

The determination of the variation of susceptibility in much weaker fields is left undecided by these experiments, but by the employment of a torsion balance we hope to be able before long to give the ratios of the susceptibility in various fields much weaker than those employed in the foregoing experiments.

In connection with the determination of the absolute magnetic susceptibility of liquid oxygen, our attention has been much directed to the important matter of the determination of magnetic susceptibilities of substances in general at very low temperatures.

Having regard to the great loss in magnetic susceptibility experienced by the ferromagnetic metals in heating beyond a certain temperature, it has been frequently suggested that bodies of small susceptibility might become strongly magnetic if cooled to a sufficiently low temperature. Faraday made many experiments on this question, using solid carbonic acid as a refrigerating agent, but was not able to arrive at any conclusions.

A difficulty which presents itself in the use of liquid oxygen as a refrigerating agent for this purpose is the strongly magnetic quality of the liquid itself. All bodies except iron, nickel, and cobalt, and the strongly ferromagnetic bodies, become apparently diamagnetic when placed in liquid oxygen and in a non-uniform magnetic field.*

Moreover, for obvious reasons it is easy to weigh a diamagnetic or apparently diamagnetic body in a non-uniform field because the forces restoring the disturbed body to its original position increase with

* An interesting experiment was made with a ball of ebonite which illustrates this fact. Ebonite is slightly magnetic in air, owing no doubt to iron impurity. Ebonite is denser than liquid oxygen. Accordingly, a small ball of ebonite dropped into the liquid oxygen contained in the vacuum vessel of the pole of the magnet sunk to the bottom of the vessel. On exciting the magnet the ebonite became apparently strongly diamagnetic and was repelled by the pole. It floated up in the liquid until it reached a level at which the diamagnetic repulsion just balanced the apparent weight of the ball in the liquid oxygen. Instructive *lecture experiments* can be made in this manner.

the displacement. If, however, an attempt is made to determine the force acting upon a paramagnetic body in a non-uniform field by a balance, the body weighed is in unstable equilibrium.

We have only recently overcome these difficulties. The method we have adopted for cooling the body under test, is to suspend it freely near the bottom of a test-tube, which is placed in a vacuum vessel, the interspace between the two being filled with liquid air. In this way the body is cooled by radiation to the temperature of liquid air, and yet it is suspended in, and surrounded by, gaseous air, the magnetic susceptibility of which is exceedingly small compared with that of liquid oxygen or liquid air.

By limiting the vibration of the balance within small limits by the stops, or by gradually varying the field of the magnet with a carbon rheostat, until the field is just able to move the object from a standard position against the fixed restraining force supplied by a constant counter-balancing weight, we have been able to effect the measurements of the apparent weight of the tested object at a given distance from the pole, and in a known field, even though the equilibrium is not stable. In this way we have made preliminary experiments on the variation in the diamagnetic susceptibility of bismuth, and of the paramagnetic susceptibility of manganous sulphate in the solid condition.

We made a preliminary experiment by weighing in and out of the magnetic field a small closed glass bulb, exhausted of its air both when in ordinary air at the normal pressure and temperature, and then suspended on the dense gaseous air in the inner test-tube, which is at a temperature of -182° C. lying at the bottom of the inner test-tube, placed as above described in a vacuum vessel. We found the magnetic susceptibility of the dense air at -182° C. to be $+0.28 \times 10^{-6}$, in other words about 10 times the susceptibility of air at the normal temperature and pressure. This number 0.28 is quite insignificant compared with numbers of the order of 100 or 300. Hence an object suspended in the above described manner, can be reduced to the temperature of liquid air without changing the susceptibility of the surrounding medium by an amount which is at all comparable either with that of liquid oxygen, or with the value of the susceptibility of bismuth, or of most paramagnetic bodies such as the salts of iron, nickel, cobalt, manganese, or of palladium, or any of the strongly paramagnetic bodies.

In this manner we have made a determination of the change in paramagnetic susceptibility of crystallised manganous sulphate in the form of powder.

We find that the susceptibility of the salt at 25° C. is to that at -182° C. in the ratio of 105 to 349 or 1 to 3.32.

These centigrade temperatures, 25° C. and -182° C., correspond to

Our experiments show that this law may be valid over very wide limits, and to very low temperatures. We hope to fully examine this matter shortly, and to make a full examination of paramagnetic susceptibility at very low temperatures.

From the mean of three experiments, we find that the diamagnetic susceptibility of bismuth is increased by cooling, and that it has the following values:—

thus showing an increase of 16 per cent. on the lower value.

The above considerations suggest that the very large paramagnetic susceptibility of liquid oxygen, which is five or six times greater than that of a saturated solution of ferric chloride, may in part be due to its low temperature.

The density of liquid oxygen is 806 times that of gaseous oxygen at 0° C. and 760 mm., and its absolute temperature is just one-third.

The magnetic susceptibility of gaseous oxygen as determined by Faraday, E. Becquerel, and others is 0.13×10^{-6} . Now $2418 \times 0.13 \times 10^{-6} = 314 \times 10^{-6}$.

The mean value we have found for the susceptibility of liquid oxygen is 324×10^{-6} , and many of our values for it are exactly 314×10^{-6} .

It seems therefore possible that for paramagnetic bodies over wide limits of density and temperature we may find that the magnetic susceptibility varies directly as the density and inversely as the absolute temperature.

We desire to add that our thanks are due to Mr. J. E. Petavel and Mr. J. T. Morris, for their assistance in carefully carrying out the tedious work of the ballistic observations, necessary to determine the field of the electro-magnet we have used.

“Aluminium as an Electrode in Cells for Direct and Alternate Currents.” By E. WILSON. Communicated by Dr. J. HOPKINSON, F.R.S. Received May 11,—Read May 26, 1898.

This paper deals with the apparent great resistance which aluminium offers to the passage of an electric current when used as an anode in cells containing, for instance, such an electrolyte as alum in water. The following are references to papers which deal in whole or in part with this or other properties of aluminium when employed as an electrode in electric cells.

Wheatstone. ‘Roy. Soc. Proc.’ Read April 26, 1855. This is the earliest paper I have found dealing with the metal aluminium in voltaic cells, but Wheatstone does not appear to have noticed the apparent great resistance mentioned above.

Heeren. ‘Mittheil. des Gewerbevereins für Hannover,’ Jahrg. 1855, p. 342. Reference is made in this paper to Wheatstone’s experiments.

Buff. ‘Liebig’s Annalen,’ 1857, vol. 102, p. 269. The author of this paper points out that nine Bunsen elements were not able to pass a current through a cell having aluminium as an electrode. This is the first mention of this property I can find.

Ducretet. ‘Comptes Rendus,’ 1875, vol. 80, p. 280; also ‘Journ. de Phys.,’ 1875, vol. 4, p. 84. Observed great resistance in dilute sulphuric acid due to aluminium plate.

Beetz. ‘Wied. Ann.,’ 1877, vol. 2, p. 94. Supposes oxygen to be the cause of this apparent high resistance.

Winkelmann. ‘Wied Ann.,’ 1883, vol. 20, p. 91.

Wright and C. Thompson. ‘Phil. Mag.,’ 1885, Part 9, Series 5, vol. 19, pp. 27, 116, 203. Call attention to the non-compliance of aluminium with thermochemical data. Reference is made to the work of Julius Thomsen.

Laurie. 'Phil. Mag.,' 1886, Series 5, vol. 22, p. 213. Investigates the effect of amalgamating aluminium, and points out the important part played by the oxide or suboxide of aluminium.

Streintz. 'Wied. Ann.,' 1887, vol. 32, p. 116; also *ibid.*, 1888, vol. 34, p. 751. Suggests a kind of dielectric polarisation distinct from ordinary electrolytic polarisation as cause of the apparent high resistance of aluminium.

Herroun. 'Phil. Mag.,' March, 1889. Refers to the disconformity of aluminium in voltaic cells with ordinary theory.

Hutin and Leblanc. 'Étude sur les Courants Alternatifs et leurs Applications Industrielles,' Part 2, Chap. 10, p. 135.

Graetz. 'Wied. Ann.,' 1897, vol. 62, No. 10, pp. 323—327; also 'Journ de Phys.,' 1898, Series 13, vol. 7. This paper specially deals with alternate currents and will be referred to again. With regard to direct currents, Graetz gives 22 volts as the electromotive force which aluminium as anode is able to oppose.

Pollak. 'Comptes Rendus,' 1897, vol. 124, p. 1443. With alkaline solutions, Pollak says he can overcome 140 volts continuous pressure. Proposes to use aluminium as one pole of a cell for the purpose of producing a uni-directional current from alternate currents.

Lang. 'Wied. Ann.,' 1897, vol. 63, pp. 191—194. Uses an electric arc with aluminium and carbon poles for the purpose of rectifying an alternate current.

PART I.

Direct Currents.

Two sizes of cells have been used in these experiments, each having aluminium and carbon electrodes. The large size consists of one aluminium plate, $\frac{1}{8}$ inch thick, and one carbon plate, $\frac{1}{4}$ inch thick, separated by ebonite bolts and nuts, the distance between the plates being $\frac{1}{4}$ inch. The surfaces thus opposed to one another in an electrolyte of saturated potash alum in water have each an area of 36 square inches. The aluminium plate was not bought as being specially pure, and may have 2 per cent. impurities. After making preliminary experiments with alternate and direct currents over a lapse of four days, the following experiment was made with this cell and was repeated.

An exploring electrode was inserted midway between the plates, and consisted of a platinum wire sealed into a glass tube. The wire beyond the tube had a length of about 2 inches, and was coiled into a small spiral, the plane of the spiral being parallel with the surfaces of the plates. A Kelvin quadrant electrometer was arranged with a *two-way switch*, so that the potential between this electrode and

either the carbon or aluminium plate could be observed. The cell was placed in series circuit with another, similar in all respects, except that the distance between the plates was $\frac{1}{8}$ inch instead of $\frac{1}{4}$ inch, an adjustable resistance, and an ammeter. By means of a reversing switch this circuit could be reversed across the poles of twenty-two storage cells having a potential difference of 44 volts. The temperature of the cell was about 12° C. The negative pole of the storage cells was connected to the aluminium plate and the current adjusted to 3.6 amperes, the potential difference between the aluminium and carbon plates being 4.4 volts, and gas evolved freely. On reversing the connections, that is, placing the positive pole of the charging battery to the aluminium plate, the current, so far as this ammeter could show, was zero, the potential between carbon and aluminium being about 22 volts. The surface of the fluid was maintained in a state of agitation, but no gas was evolved, except in very small quantity, if any.

The figures in Table I show how the potentials between the

Table I.

Reversal from Al anode to Al cathode.				Reversal from Al cathode to Al anode.		
Time in minutes after reversal.	Volts between Al plate and exploring electrode.	Volts between C plate and exploring electrode.	Amperes.	Volts between Al plate and exploring electrode.	Volts between C plate and exploring electrode.	Amperes.
0	+16.6	+1.63	0	-1.74	-2.73	-3.56
About $\frac{1}{2}$	-3.81	-2.04	-3.35	+14.3	+0.30	+1.20
1	-2.48	-2.40	-3.42	+18.2	+1.57	+0.64
2	-2.23	-2.79	-3.47	+19.1	+1.64	0
3	-1.9	-2.84	-3.49	+19.6	+1.67	0
4	-1.9	-2.84	-3.50	+20.0	+1.68	0
5	+20.5	+1.67	0
7	-1.65	-2.84	-3.55			
13	-1.74	-2.73	-3.56			
190	+17.9	+1.76	0

exploring electrode and the carbon and aluminium plates respectively, as also the current, varied in terms of time after reversal took place. The time between the two reversals in Table I was 27 minutes. There is no doubt but that the current, when reversal took place from 3.6 amperes, first crossed the zero and acquired an opposite sign, finally coming to zero of the instrument.

This is an important point, and was fully established during

another set of experiments undertaken to find the resistance of the electrolyte. For this purpose the plates were separated to the extent of $1\frac{5}{8}$ inch, and two platinum exploring electrodes used, the distance between them in the electrolyte being $\frac{1}{8}$ inch, along a straight line perpendicular to the parallel surfaces of the plates. Each time reversal took place from negative to aluminium, to positive to aluminium, the potential between these electrodes changed sign, and gradually returned to near zero. The results of the experiments on resistance show that at this temperature the resistance of a layer of electrolyte of area equal to the area of the plate submerged, and of length equal to the distance between the electrode and the plate, that is $\frac{1}{8}$ inch in the experiment of Table I, is 0.063 ohm. The correction is therefore small with current 3.6 amperes, and is negligible when the positive pole is connected to the aluminium.

The next set of experiments were carried out with aluminium $\frac{1}{8}$ inch thick, of 99.5 per cent. purity, the electrolytes being specially pure, and only distilled water used. The cells used are of a smaller size, and each consists of one aluminium plate, $1\frac{1}{2}$ inches wide, having a carbon plate on each side of it, the distance between the aluminium and carbon being $\frac{5}{8}$ inch, the carbon plates being $2\frac{1}{2}$ inches wide and $\frac{1}{4}$ inch thick. The aluminium plate was submerged $2\frac{3}{4}$ inches in the electrolyte, so that the total area for current is $8\frac{1}{4}$ square inches.

Two such cells were prepared, one with a 10 per cent. by volume solution of H_2SO_4 in water, the other with a saturated solution of potash alum, and left for forty-seven hours with a current of $\frac{1}{4}$ ampere passing through each in series, the positive of charging cells being connected to aluminium. At the end of this time, and with this current, the potentials across the cells were 2.4 volts for the H_2SO_4 solution, and 9 volts for the alum solution. On breaking the circuit, the potential of the H_2SO_4 solution cell fell to about 0.38 volt in one minute, and rose fairly gradually to 2.4 volts on making circuit. The alum solution cell lost its potential immediately on breaking circuit, that is, the electrometer needle appeared to return to zero, as though there were no opposing electromotive force. When the current was reversed, that is negative to aluminium, and still of the value $\frac{1}{4}$ ampere, the H_2SO_4 solution cell gave 0.24, and the alum solution 1.29 volts.

The aluminium plate which was formed in the H_2SO_4 solution, together with its carbon plates, were next washed in distilled water, and placed in a saturated solution of alum. Eleven storage cells were connected without extra resistance in circuit to each of these cells positive to aluminium, and the resulting current noted. The plate which had been formed in H_2SO_4 allowed about 0.2 ampere to pass, and in 4 hours 10 minutes this current had risen to about

1 ampere, the temperature of the cell having risen. The plate formed in alum solution only allowed 0.083 ampere to pass. The two cells were then placed in series and left for 13 hours, with $\frac{1}{10}$ ampere passing from the aluminium to carbon plates in each.

The following test was then made. The cells had opposed to them, aluminium to positive, in turn from 1 to 20 storage cells, without extra resistance, rising a cell at each step, the resulting current and potential across the terminals being noted. The results are given in Table II. We see that from 1 to 18 storage cells the

Table II.

Number of storage cells applied.	Plate formed in dilute H_2SO_4 .			Plate formed in alum solution.		
	Volts across cell.	Amperes.	Temperature C.	Volts across cell.	Amperes.	Temperature C.
1	1.89	0.0005	13	1.89	0.0009	13
2	3.78	0.026	..	3.87	0.026	
3	5.67	0.034	..	5.76	0.036	
4	7.65	0.036	..	7.56	0.053	
5	9.54	0.036	..	9.45	0.053	
6	11.3	0.050	..	11.3	0.077	
7	12.6	0.055	..	12.6	0.062	14
8	14.4	0.062	..	14.4	0.098	
9	17.1	0.069	..	16.2	0.108	14.5
10	19.8	0.070	..	19.8	0.12	15.5
11	21.6	0.079	..	21.6	0.13	15.7
12	23.4	0.089	..	23.4	0.144	16
13	25.2	0.096	..	25.2	0.161	17
14	27.9	0.120	..	27.9	0.178	
15	29.2	0.127	20	29.2	0.20	18
16	31.5	0.191	..	31.5	0.191	
18	34.2	0.987	..	35.1	0.34	
20	39.1	increased rapidly to 4 amperes, circuit then broken	21½	39.6	increased rapidly to 1 ampere, circuit then broken	19½
16	29.7	0.9	23	30.6	0.29	

currents gradually increase in each, the plate formed in H_2SO_4 having an apparent greater resistance, up to about 16 cells. With 20 cells applied the H_2SO_4 -formed plate gave way with great rapidity, and in a very short time, about fifteen seconds, the current was 4 amperes, the temperature of the cell rising also from 21° C. to 23° C. The alum-formed plate seemed more stable with 20 cells, but speedily allowed a current of over 1 ampere to pass. On going back to 16 cells the currents were 0.9 ampere in the H_2SO_4 -formed plate, and 0.29 ampere in the alum-formed plate.

This brings us to the effect of temperature upon potential for a given current. The cell containing the aluminium plate formed in alum solution above referred to was placed in an oil bath, the temperature of which could be varied. The current was that due to 56 storage cells, through a considerable external resistance, about 650 ohms during heating, and 2280 during cooling. There was an excess of alum in the cell, and the solution was kept saturated. The variation of potential between the aluminium and carbon electrodes was noted, as also the temperature of the cell. The results are given in Table III.

Table III.

Time.	Ampere.	Volts.	Temperature C.	Remarks.
h. m.				Current switched on
0 0	0.16	rising	13.5	+ to Al
0 5	0.124	29	"	"
0 10	"	30	"	"
0 15	0.169	1.3	14	- to Al
0 16	0.122	32	"	+ to Al
1 55	0.132	26.1	20.4	"
2 0	0.172	1.1	"	- to Al
1	0.124	31	"	+ to Al
8	0.132	24.7	25	"
12	0.139	20.2	29.5	"
20	"	11.7	36.5	"
22	0.141	7.65	43	"
27	0.143	7.2	52	"
28	0.155	1.1	"	- to Al
30	0.141	7.2	56	+ to Al
33	0.143	5.4	61	"
37	0.148	3.0	70	"
44	0.0518	0.18	72	"
3 15	"	0.72	63	"
25	"	1.3	56.5	"
31	0.0509	2.3	51	"
50	"	2.7	40	"
22 50	0.048	10.3	12	"

We see that as the temperature of this cell rises from 13.5 to 70° C., the potential difference falls from 30 to 3 volts. The experiments already made on the resistance of the electrolyte will only account for 0.043 volt at 13.5° C., and 0.025 volt at 57° C. with the currents 0.124 and 0.141 ampere as given in Table III. The conclusion is that temperature has an effect upon the apparent high resistance of an aluminium plate and its film, the subject of this paper. This points to the fact, that in practice for high apparent resistance, it would be necessary to cool or circulate the electrolyte with such dissipation of energy, that the cell would otherwise acquire a high temperature.

In Table III we see that as the cell cools with a smaller current of about 0.051 ampere, the potential between the aluminium and carbon plates rises. In this case at 56, and 12° C., the electrolyte would account for 0.0093 and 0.017 volt respectively.

It was thought that if a plate of aluminium with its film were submerged in mercury, the resistance between the metal and mercury might give an idea as to whether resistance, pure and simple, played an important part in the effects observed. The plate originally formed in H_2SO_4 solution was carefully removed, washed in distilled water and dried, and half submerged in clean mercury. Storage cells ranging in number from 1 to 15 were applied as in Table II, but in each case the poles were also reversed so as to test the insulation with the two directions of currents. The results show that this film on aluminium is a fairly high insulator, but it was not stable. At times the resistance was zero, when a sharp noise occurred in the cell like sparking between points in air, and the insulation was immediately restored. With 16 volts applied, the apparent resistance was about 10,000 ohms, whereas from Table II we see the apparent resistance of the whole cell is 230 ohms at 14 volts, the positive pole being connected to aluminium. I should say, judging from the number of times the film broke down, that it was more stable when the positive pole of the charging battery was connected to the aluminium; but in either direction the resistance, when established, had the same order of magnitude. Up to six cells, no extra resistance was included in the circuit as the film was stable, after this a resistance was inserted so as to keep down the current when the film broke down, as then the potential between Al and Hg was zero. Even at 30 volts the film was able to restore its insulating properties, but very rarely. On removing the plate, a film was left on the mercury where it had been in contact with the film on the aluminium plate.

Another set of experiments was made with two cells having as electrolytes a 5 per cent. solution of H_2SO_4 in water, and a saturated solution of potash alum in water. The area of the pure aluminium plate exposed to the fluid was 17 square inches in each cell. A current of 1 ampere was passed for four hours through the H_2SO_4 solution cell, and three hours through the other. At the end of these times the temperatures were respectively 33 and 51° C. With the 1 ampere passing from Al to C, the potentials between the plates were respectively 6.3 and 20 volts. The cell containing the plate formed in potash alum solution was then heated, the current through it being kept fairly constant by means of a considerable external resistance and 110 volts. It was then cooled by placing it in a freezing mixture of ether and carbonic acid snow. The temperature in this case was reduced somewhat rapidly, and a portion of the electrolyte at the bottom was frozen, probably a cryohydrate, leaving liquid above.

The temperature of this liquid portion did not fall below zero centigrade. The results are given in Table IIIA. When comparing these

Table IIIA.

Time.	Amperes.	Volts across one cell.	Tempera- ture.	Remarks.
<i>Heating.</i>				
			° C.	
12.45 p.m.	0.154	28.8	17	Al connected to positive pole of charging cells.
1.0 "	0.154	27.0	18	
3.40 "	0.174	15.3	24	
4.20 "	0.170	16.2	25	
6.40 "	0.187	16.6	26.5	
<i>Cooling.</i>				
11.0 a.m.	0.161	29.2	13	The temperatures are those of the liquid electrolyte.
11.3 "	0.160	30.1	11	
11.35 "	0.158	30.8	3	
12.0 noon.	0.160	31.0	1	Bottom portion of electrolyte found frozen.
12.30 p.m.	0.168	26.3	2	
1.0 "	0.170	25.2	2	Al to negative.
1.10 "	0.055	4.95	2	
1.12 "	0.058	1.35	2	Al to positive.
1.14 "	0.235	2.25	2	
1.15 "	0.165	26.6	2	Fluid portion of electrolyte poured away, only frozen portion remain- ing.
2.50 "	0.158	31.2	not known	

with the results of Table III, one must remember that the areas of the plates in the electrolyte are 17 square inches in Table IIIA and $8\frac{1}{4}$ square inches in Table III. This paper does not deal with the chemistry of the effect discussed. It is known that a clean aluminium plate acquires this film, when simply submerged in alum solution, in the presence of oxygen, without the passage of currents. We have seen that with a given film time is required to develop the effect.

[Note added 19th May, 1898.

The film on these plates has been examined by Mr. Herbert Jackson, of the Chemical Department, King's College, London, and the following is his communication.

" King's College, London,
" May 12, 1898.

" DEAR WILSON,

" I waited to send you the results of my examination of the *aluminium plates* until I had looked at them carefully with the

microscope. Not much information, however, is to be gained from this. The skin over the plates is, however, seen to be full of minute cracks in every direction, giving the impression of a dried gelatinous pellicle; not an unexpected appearance if the plate had been covered when wet with a thin coating of the gelatinous aluminium hydroxide. The analysis of the film over the metal shows it to consist of basic aluminium sulphate. The origin of this may of course have been the formation in the first place of aluminium hydroxide which subsequently reacted with some of the alum solution to give the basic compound. How far the formation of this may have anything to do with electrolysis would, I should think, be difficult to say without further and more elaborate experiment, and it must be remembered that a similar coating can be obtained on an aluminium plate by immersing it in an alum solution and leaving this freely exposed to the air.

"Yours sincerely,

"HERBERT JACKSON."

I have tried an experiment which I think shows that a film formed on aluminium by first being placed in contact with an alum solution and then exposed to the air, gives the same effect as a film formed in the cell when a current is passed from the aluminium to a carbon plate through the electrolyte. Two similar bright aluminium plates were prepared with carbon plates on either side of them, the area of aluminium in the electrolyte being $8\frac{1}{4}$ square inches. The electrolyte consisted of a saturated solution of potash alum in water. One plate was left in the solution for seven hours with no current passing, and then exposed to the air for $16\frac{1}{2}$ hours. The other plate was submerged in the fluid and immediately readings were taken of the current passing and potential difference in volts between the aluminium and carbon plates. The effect in this case was exactly what was previously observed. That is to say, the current being maintained constant at 0.055 ampere by about 2000 ohms being inserted in circuit with the cell across about 110 volts, the potential difference immediately passed from a small negative value through zero, and after three minutes and forty minutes, had respectively the values 1.8 and 2.27 volts; the temperatures being $12\frac{1}{2}$ and $15\frac{1}{2}$ ° C. The test was continued. For the next $5\frac{1}{4}$ hours the current was 0.204 ampere, it was then dropped to 0.055 ampere and kept at this value for $16\frac{1}{2}$ hours. At the end of this time the potential was 10 volts with 0.055 ampere, and 28.6 volts with 0.163 ampere, the temperature being 15° C.

The other plate was submerged in its solution, the potential difference and current being immediately noted as before. The results in this case show that the potential, with 0.054 ampere, rose immediately

from a small negative value of about 0.2 volt to a positive value of 1.8 volts. After four minutes and forty minutes respectively the potential with current 0.054 had the values 2.07 and 2.56 volts, the temperatures being 13 and 15.5° C. This, I think, shows that a film formed by exposure to the air after being submerged in a saturated alum solution, has the same effect as another formed in this electrolyte during the passage of current. This test was continued. After twenty-four hours, during which the current was 0.0523 ampere, the potential was 11 volts, with this current passing, and the temperature 16° C.

The two cells were then placed in series and an average current of 0.048 ampere passed through them from the aluminium to carbon plates for fifty hours. At the end of this time, with current 0.048 ampere, the potentials were 10 in the case of the plate partially formed without current, and 10.3 in the case of the other; the temperatures being 19 and 17½° C. respectively. With current 0.121 ampere, the potentials were 26.2 and 27.4 respectively at temperatures 21 and 19½° C.]

Sodium hydrate forming a weak solution in water was tried as an electrolyte, the area of the Al plate being the same as before, 17 square inches. The forming current of 0.8 ampere was passed from Al to C for 2 hours 20 minutes, when the potential between the plates was 13.6 volts, the aluminium plate being covered with a thick black deposit.

PART II.

Alternate Currents.

The experiments with alternate currents were undertaken in order to investigate the instantaneous values of potential and current. One object was to see if the effect we have dealt with in the first part of this paper has time to properly develop with ordinary frequencies, and if so under what conditions. Another object was to see if aluminium is a valuable metal for use in condensers for alternate currents.

Aluminium-Carbon Cells.

If the time taken to develop the effects dealt with in the first part of this paper were very small compared to the time of a complete period of an alternating potential applied to the cell, one would expect to get a practically uni-directional current in the circuit of the cell under favourable conditions as to temperature and applied potential. This is not the case with the cells and frequencies dealt with in this paper. In all these experiments the author has *endeavoured to make the cells the controlling part of the circuit,*

that is to say between the terminals of the alternate current machine a small non-inductive resistance and a Siemens dynamometer were the only part of the circuit other than the cells experimented upon. The copper resistance of the circuit including armature was from 1 to 5 ohms.

In the first portion of these experiments, the results of which are given in Table IV, the instantaneous values of the current, and the

Table IV.

Experiment.	Frequency.	First half period.			Second half period.			Ratio of maximum coulombs.	Average watts per period.	Amperes given by Siemens dynamometer.	Average temperature of cell C.
		Maximum volts.	Maximum amperes.	Phase difference $360^\circ =$ one period.	Maximum volts.	Maximum amperes.	Phase difference $360^\circ =$ one period.				
(1)	92	8.9	5.2	24°	26	4.0	56°	1.35	26.8	3.28	27
(2)	91	7.8	1.6	57	8.9	1.5	69	1.07	2.65	1.10	9
(3)	91	14.2	16.2	6	33	9.4	42	1.97	120	8.99	26
(4)	74.5	21.1	39.8	3	18.6	26.5	3	1.78	294	21.0	36
(5)	52	13.4	14.5	6	20.2	9.6	24	1.54	86	8.16	35

potential difference between the terminals of the cell, were observed by aid of a Kelvin quadrant electrometer and a revolving contact maker. The cell across which the potentials were observed consisted of one aluminium plate $\frac{1}{16}$ inch thick separated by $\frac{1}{8}$ inch from a carbon plate $\frac{1}{8}$ inch thick, the electrolyte being a saturated solution of potash alum in water. The surfaces thus opposed to one another in this electrolyte have each an area of 36 square inches. Another cell of the same size as the above was placed in series with this cell. Four smaller cells were used in some of the experiments as will be set forth. Each of these consists of a thin aluminium plate opposed to a carbon plate, the opposed surfaces in a saturated solution of alum have each 8 square inches area, the distance between such surfaces being $\frac{3}{4}$ inch.

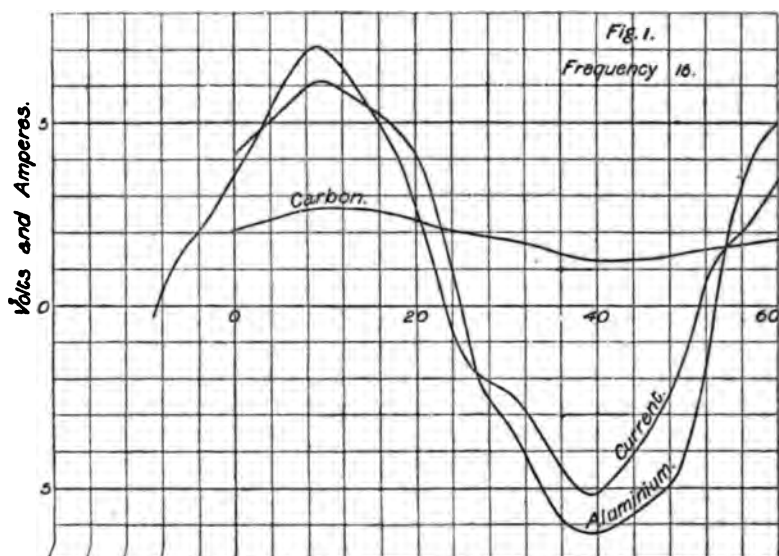
In Table IV the arrangement of these cells was as follows:—Experiments (1) and (2) all the six cells in series. Experiment (3) the two large cells in series with one another and with the four small cells arranged 2 series 2 parallel. Experiments (4) and (5) only the two large cells in series were used. No attempt was made to cool or circulate the electrolyte, but it had considerable volume and took some time to heat and cool. In all cases there was an excess of salt to insure saturation. Table IV contains the important information obtained from the tests.

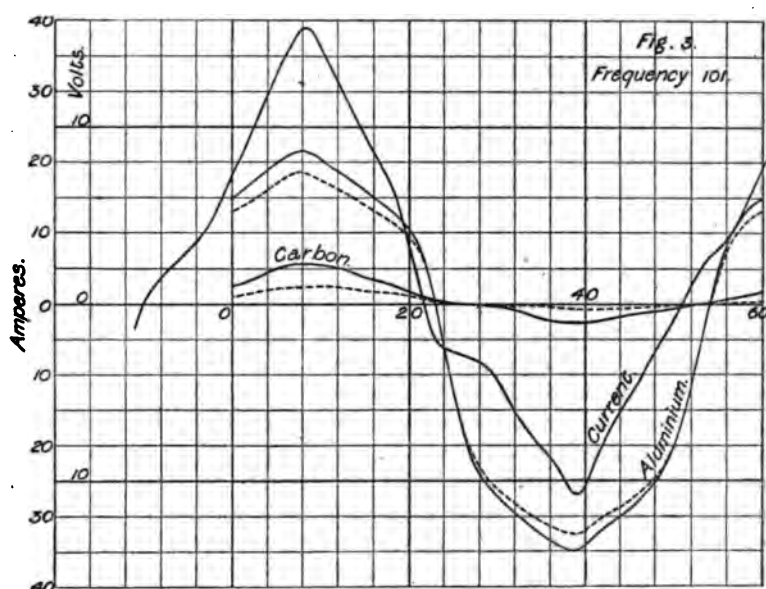
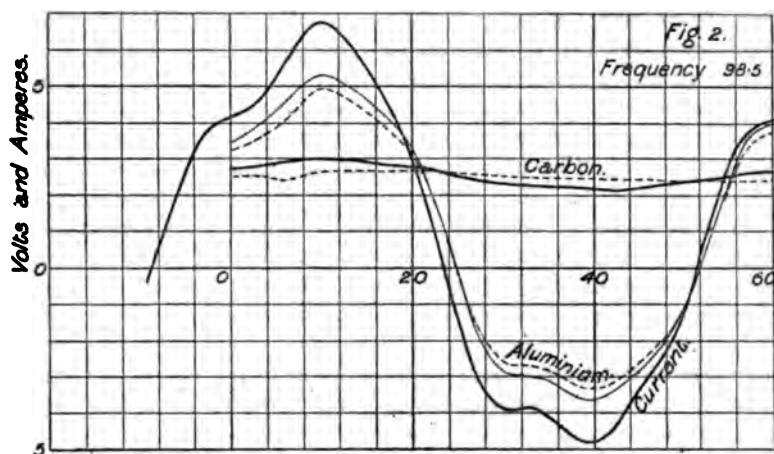
These figures show that small currents are accompanied by large

phase difference but the effect we are looking for, namely, a large ratio between the maximum coulombs in the two halves of a period, mainly develops with large currents for a given frequency accompanied by high temperature and small phase difference. The average watts have been deduced from the product of instantaneous volts and amperes at twenty equal intervals during a period.

The experiments in figs. 1, 2, 3 give results obtained with an exploring electrode inserted between the plates in the electrolyte as in the first portion of this paper. The aluminium and carbon plates in one of the large cells were separated to $\frac{1}{4}$ inch, and the two cells kept in series as in experiments (4) and (5) Table IV. The curves refer to this cell, and provision was made for obtaining the instantaneous values of the current in the cell, and the potentials between the exploring electrode and either the aluminium or the carbon plate.

In figs. 1, 2, the Siemens dynamometer in the circuit registered the same current, namely 3.97 amperes, but the frequencies are 16 and 98.5 respectively. We see that $\frac{1}{16}$ second is too short to allow the effect we are looking for to fully develop, since the ratio of the maximum coulombs is 1.47. On the other hand, if we examine the two sets of curves we see that at 98 periods per second the potential difference between the exploring electrode and aluminium has a maximum value of 3.63 volts during the half period when the coulombs have the smaller maximum, whereas at 16 periods per second, the same maximum current is produced with a larger





maximum potential, namely, 6.27 volts. That the aluminium plate was an anode during the half period when the maximum coulombs were a minimum, was proved by noting the direction of the electrometer deflection, when the positive pole of a Clark's cell was connected to the insulated quadrant, and examining the observed direction of the deflection during the experiment. Experiments made upon the resistance of the electrolyte with two exploring electrodes, employing alternate currents, show that at the tempera-

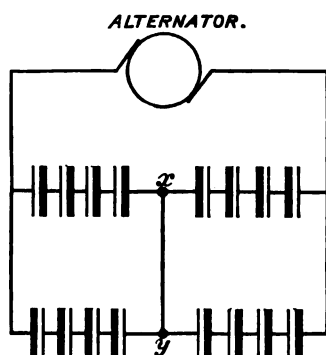
ture at which these experiments were made, namely, $13\frac{1}{2}^{\circ}$ for the 16 frequency, and $11\frac{1}{2}^{\circ}$ C. for the 98.5 frequency, the resistance of a layer of the electrolyte of area equal to the area of the plate submerged, and length equal to the distance between the electrode and the plate, has a value of about 0.063 ohm. The curves of potential in fig. 2 have been corrected for this, and the dotted lines show the results. The average watts dissipated by electrolytic hysteresis at the carbon and aluminium plates at 98.5 periods per second, fig. 2, are 0.69 and 4.5 respectively. This takes no account of the resistance of the electrolyte, and has been obtained by taking instantaneous products of potential and current at twenty equal intervals during the period. During the first half-period work is supplied at the carbon plate on the average at the rate of 4.1 watts; during the second half-period the plate does work on the system at the average rate of 2.7 watts. The aluminium plate returns practically nothing to the system.

In fig. 3, the frequency is 101, the ratio of the maximum coulombs in the two halves of the period is 1.7, and the Siemens dynamometer in the circuit registered 20.6 amperes. The average temperature during this experiment was about 55° C., but had a maximum of 64° C. at the end of the test. Under these conditions, evaporation of the electrolyte was rapid. The experiments on the resistance of the electrolyte at this frequency and temperature give 0.033 ohm, and the potential curves have been corrected, the result being shown by the dotted lines. The average rate of dissipation of energy due to electrolytic hysteresis at the aluminium plate during the period is 153 watts, whereas at the carbon plate this rate is only 9.8 watts. We see that the maximum volts during the half period when the maximum coulombs are a minimum have risen to 13, whereas during the other half period the maximum volts are only $7\frac{1}{2}$.

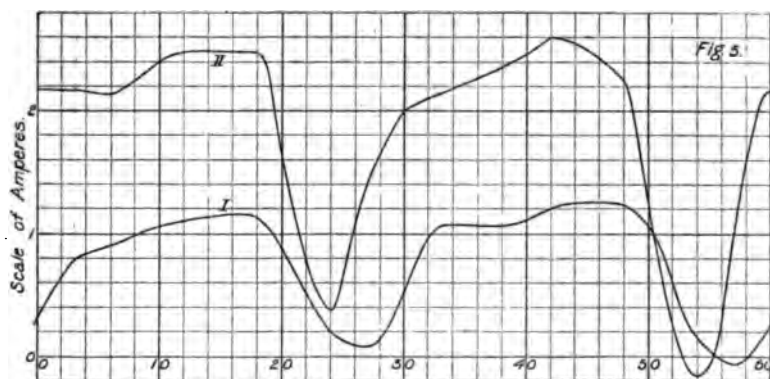
The paper by Graetz already alluded to, deals with alternate currents; and two groupings of cells with aluminium plates as the one electrode are given, whereby he proposes to rectify alternate currents. He gives 22 volts as the potential an aluminium cell is capable of opposing, and states that the current could not be measured with a delicate galvanometer. The first grouping of these cells consists of placing between the poles of the alternator two circuits in parallel, in each of which he places four cells in series. The poles are reversed so that in the one circuit the predominating current will be positive, whereas in the other it will be negative. The other grouping of these cells is shown in fig. 4, and the author states that he gets a unidirectional current in the circuit *xy*. In this circuit he has operated a direct current motor, and deposited copper. By superposing one of the current curves in figs. 1, 2, or 3 on the same curve, but with reversed phases, one can form an idea

as to what the resulting current would be in the circuit *xy*. Curves in fig. 5 were obtained by observing the instantaneous value of the potential difference between the ends of a non-inductive resistance of 0.349 ohm, forming the circuit *xy*, fig. 4. Four small cells were

FIG. 4.



used, each one taking the place of the group of four in the diagram ; each of these consists of a thin aluminium sheet and a carbon plate, the opposed surfaces in a saturated alum solution having each 10 square inches, separated by a distance of $\frac{5}{32}$ inch. In fig. 5,



curve I, is the current in *xy*, when the current from the alternate current machine through the system was 3.96 amperes, as given by a Siemens dynamometer. Curve II is the current in *xy*, when the Siemens dynamometer read 10.4 amperes. The frequencies were 74 and 73.3, and the temperatures of the cells 25° C. and 44° C. respectively in the two experiments. Graetz states that with cells

of sufficient size 95 to 96 per cent. of the energy of the alternate current can be changed into direct current. The efficiency of such a system as shown in fig. 4 will obviously be the ratio of the rate at which work is done on *xy* to the rate at which work is done on the whole system by the alternate current machine. If this is to be 95 per cent., then only 5 per cent. must be dissipated in the cells.

An important point in connection with the working of these cells is the wearing away of the aluminium. The thin metal used in the small cells above alluded to is perforated with small holes, but I have not noticed so much deterioration in the thicker sheet. The evaporation of the electrolyte is another matter which needs consideration if the temperature is raised fairly high in working.

Two Aluminium Plates in Alum Solution.

These experiments were undertaken to find what effect as a condenser this metal with its film has with varying frequency temperature and current. The aluminium plates in the two large cells above experimented upon were opposed to one another in a saturated alum solution. The distance between them was $\frac{1}{8}$ inch, and the opposed areas in the solution are 36 square inches on each plate.

The important data have been collected in Table V. As a com-

Table V.

Experi- ment.	Fre- quency.	Max. volts across one cell.	Max. amperes.	Current $\sqrt{\text{mean}}$, as given by Siemens dyna- mometer.	Temperature.	Ratio of energy returned to en- ergy supplied per period per cent.	Average phase difference, 360° = one period.	Average rate of dissipation of energy in cell.	Plates.
				Amperes.	° C.			Watts.	
(a)	96	25.0	30	21.6	82	—	12°	—	Al
"	"	3.35	"	"	24	—	9	—	Fe
(b)	17	46.0	8	3.93	53	0.72	12	150	Al
"	"	1.3	"	"	11	—	60	—	Fe
(c)	100	52.5	2.6	1.3	14	16.0	54	42	Al
"	"	0.25	"	"	9	—	—	—	Fe
(d)	92.4	21.0	1.48	"	11	11.2	60	9.24	Al
(e)	92.4	8.0	0.51	—	13½	16.6	57	1.11	Al

parison two plates of ordinary sheet iron of the same area and the same distance apart as in the aluminium cell were placed in a saturated solution of alum and placed in series with the aluminium cell. As before the circuit included a Siemens dynamometer and non-inductive resistance, and potentials were observed for different positions of the phase across the non-inductive resistance, and each cell, by aid of a Kelvin quadrant electrometer and revolving contact maker. *On account of the irregular wave form of the curves of potential and*

current, the ratio per period of the energy returned to the system by the cell to the energy supplied to the cell, is given as a percentage. If the electrolytic process were perfectly reversible, we should expect the curves of current and potential to have a phase difference of one quarter period, if they were sine curves. An examination of Table V shows that maximum phase difference develops with the smaller currents at lower temperature. In experiment (c) the maximum amperes are 2.5, the maximum volts 53. The curve of potential difference has a maximum rate of change of about 57.5 volts in $\frac{1}{600}$ second, so that an ordinary condenser with maximum current 2.5 amperes would have a capacity of about 72 microfarads. We see, therefore, that aluminium is suited for the plates of condensers. The average watts in Table V have been deduced from the instantaneous product of potential and current at twenty equal intervals during a period.

The foregoing experiments employed saturated potash alum solution as electrolyte. The following experiments deal with soda, ammonia, and potash alums, first when the solutions were saturated, and second when non-saturated. In each of the non-saturated solutions the proportions were thirteen parts of saturated solution at about 12° C. by volume and seventy equal parts of distilled water. Three cells were constructed, each containing two aluminium plates of 99.5 per cent. purity, separated $\frac{1}{8}$ inch apart, and each having 33 square inches of surface in the electrolyte opposed to the other. The results of the experiments are given in Table VI.

With regard to the saturated solutions one may say that at the low frequency 7.5 Table VI the results are not so good as at the high frequency 92.4 Table V. The non-saturated solutions also show a better result with regard to efficiency at the higher frequency.

These plates were not specially formed with direct currents and carbon cathodes before starting the above experiments. In a preliminary experiment of about one hour's duration, before the first series at frequency 33 in Table VI were made, and starting with clean polished plates, the maximum volts for the soda, ammonia, and potash were, a few minutes after starting, 4.4, 21.5, and 32. The maximum current was 0.48 ampere and the phase differences in each case about 70°. The frequency was 23 and the temperature of each cell about 14° C.

An experiment was tried in which two aluminium discs 6 inches diameter and separated $\frac{1}{8}$ inch on an ebonite spindle were submerged to within $\frac{3}{8}$ inch of the centre in a saturated potash alum solution and rotated at 108 revolutions per minute by a small electric motor. In this manner more than half the discs were continuously exposed to the atmosphere. Two brushes bearing on copper discs,

pressed into good contact with the aluminium discs, served as a means of transmitting current through the electrolyte between the discs. The frequency was 73, the temperature 18°C. , and square root of mean square value of current about 1 ampere. No perceptible difference was observed in phase difference between potential and current when the discs were rotated and at rest in the electrolyte.

The conclusion is that the effect investigated in this paper takes time to develop, and is not fully developed with alternate currents of frequencies sixteen and ninety-eight complete periods per second. It can be increased by increasing the current density for a given film, and is greatly influenced by temperature. The metal aluminium with its film is suitable for use as the plates of condensers, if due regard be given to current density and temperature. It might in some cases be found useful as an equivalent to a metallic resistance.

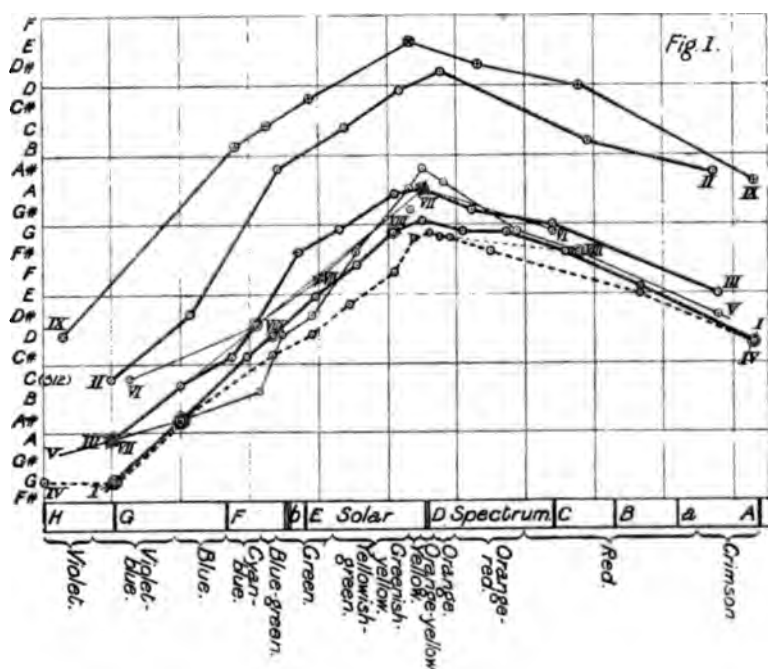
Messrs. Simpson, Greenbank, and Davy, Student Demonstrators in the Siemens Laboratory, King's College, London, have given me valuable assistance in the experimental part, and in the working out of results. To these gentlemen I tender my thanks.

"Contributions to the Study of 'Flicker.'" By T. C. PORTER,
Eton College. Communicated by LORD RAYLEIGH, F.R.S.
Received May 13,—Read May 26, 1898.

Much work has already been done on this subject, though little of a quantitative character. Many observers have described the curious colour sensations which rapid alternations of light and darkness can excite under certain conditions, admirably exemplified in the "spectrum tops." Foremost amongst those whose experiments and writings have led to the present very general interest in the subjects of flicker, and of the sensation of light and colour, may be mentioned Helmholtz, Silvanus Thompson, Shelford Bidwell, Henry, Charpentier, and Rood; whilst the first to try experiments on the relative sensitiveness of the eye to flicker in light of different colours, seems to have been J. Plateau, who, however, employed pigments, and not the colours of the spectrum.

The writer's first experiments were made to ascertain the exact relative rotations at which the flicker just vanishes in the different colours of the same spectrum, and were carried out (a) as suggested by Professor Rood in his 'Modern Chromatics,' with a balanced, blackened, opaque disc, having a broad semicircular arc removed, and (b) on a cardboard disc, half black, half white, viewed in the different colours of the spectrum of the second order of a Rowland's plane diffraction grating of 14,434 lines to the inch. Two sources of light were employed, (a) direct sunshine, (b) lime-light. The results

are conveniently expressed by curves (fig. 1); along one axis of which the spectrum is plotted, on the other the rate of rotation of the disc.



This last was found from the pitch of the note, obtained by blowing air gently through a known number of equidistant holes pierced in the circumference of the disc, and comparing these notes with those of a set of standard forks. Where the lime-light was used, great pains were taken to keep the intensity of the light constant throughout each experiment, though in these first experiments the intensity varied for the different curves. It may be well to point out that owing to the nature of the spectrum of lime, the orange is somewhat brighter and the blue and violet darker, relatively, than they are in sunshine. It is for a similar reason, as well as for others too obvious to mention, that the arc light was not employed. It will be seen, that in fig. 1, the intervals of the chromatic scale are plotted in equidistant order, and it should be remembered that the arithmetic increase in the number of revolutions to raise the pitch of the note from C (128 vibrations) to C (256), is only half that to raise it from C (256) to C (512), and so on. The effect of plotting on the axis of Y the actual number of revolutions, would be to make all the curves a great deal steeper. It seems necessary, too, to mention

that the writer's eyes are perfectly normal, and that he has a good ear. There are so many experimental details, that to describe fully a single observation, and to state the reasons for the many precautions which must be taken if the result is to be of any value, would take up so much space that the writer thinks it best only to mention a few of the more important:—The same spot on the lime was never used twice; the width of the slit was kept unaltered; each disappearance of flicker was witnessed by two, sometimes three, observers; the same time as nearly as possible was maintained between the observations, and used in making them, in order that the retina as well as the lime might be in approximately the same condition for each; the room was completely dark* and the eyes rested between the observations, never looking at any bright objects, and, above all, not at the lime; the oxygen and coal gas were conveyed by special metal pipes from gasometers in which they were stored under constant pressure outside the building, but since the composition of both gases is liable to variation, the intensity of the light was maintained constant, by regulating the supply of the gases till the flicker of a half and half disc in the yellow vanished at the same speed of rotation (generally A'). After every set of experiments, to make sure that the illumination had not sensibly changed, the note for vanishing flicker in the yellow was again observed. Experience teaches that the blue-green is rather better than the yellow for this purpose, and accordingly it was used in later experiments. Not more than two curves, often only one, were drawn in twenty-four hours, and to check the effects of contrast in consecutive experiments in different colours, a curve was drawn (a) by taking the colours of the spectrum from red to violet and *vice versa* in their natural order, and (b) by finding the pairs of colours on either side of the yellow for which flicker vanished at the same rate of rotation. The results of experiments made thus agree very closely, and the writer may say here that throughout the eight years during which the research has been carried on, the feeling has steadily grown that in subjective vision we have to do with immutable quantities, capable of exact measurement, and which vary but slightly, if at all, with different people.

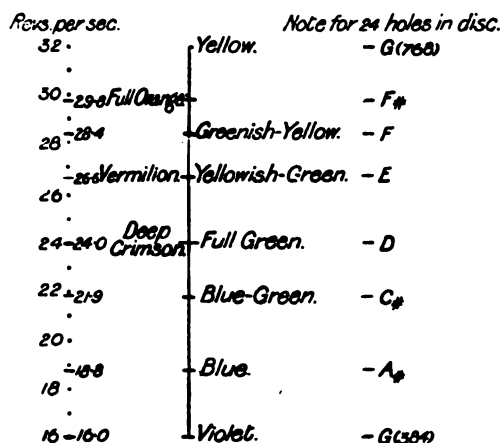
A glance at the curves in fig. 1 will show that there is a very great difference in the rates of rotation necessary for the disappearance of flicker in the different parts of the spectrum, the speed for the yellow being very nearly *double* that for the violet.

Also, that on each side of the yellow the rate decreases, until the flicker disappears for the last distinctly visible red at the same rate at which it vanishes for the full green.

Fig. 2 presents this last fact in a somewhat different way, and, if

* So far as external light is concerned.

FIG. 2.



anything, more clearly; the yellow and the violet—the two extremes so far as the present considerations go—are placed at the top and bottom of the central vertical line, and the number of rotations per second necessary for flicker just to vanish, given in the column on the left, is seen to be (for *this half-white, half-black disc*) 32 for the yellow, and 16 for the violet, and 24 for the last pair of colours—crimson and full green. The comparative rates of rotation for these colours thus bear the ratios of 2 : 3 : 4, which are easily remembered.

To return to fig. 1. The curves II and IX lie considerably above the others: they are the expression of observations made, as suggested to Professor Rood by Dr. Woolcott Gibbs, by viewing the spectrum through a rotating disc having a sector of 180° removed, using a telescope. Though the *form* of these two curves is practically the same as that of the others, their higher position, due to the superior intensity of the light received by the eye in this direct vision method, proves clearly what is already well known for white light, that the speed necessary for the disappearance of flicker increases with increase of the intensity of the light, whatever its colour may be. No. II is for lime-light, No. IX for sunlight, the rays of the sun being reflected through the slit by means of a heliostat.

Of the other curves in fig. 2 not much need be said; they are all for lime-light, the spectrum being thrown on the rotating cardboard disc half white and half black. The writer has thought it better not to smooth the curves in any way.

These curves, then, not only confirm the already known fact that

the stimulus given to the retina lasts *undiminished* a shorter time for the yellow than for any other colour, but they give the very approximately exact relative "last" for the different colours. The general form of the curves following very closely the curve which expresses the luminous intensity of the spectrum obtained by Newton, Abney, and others.

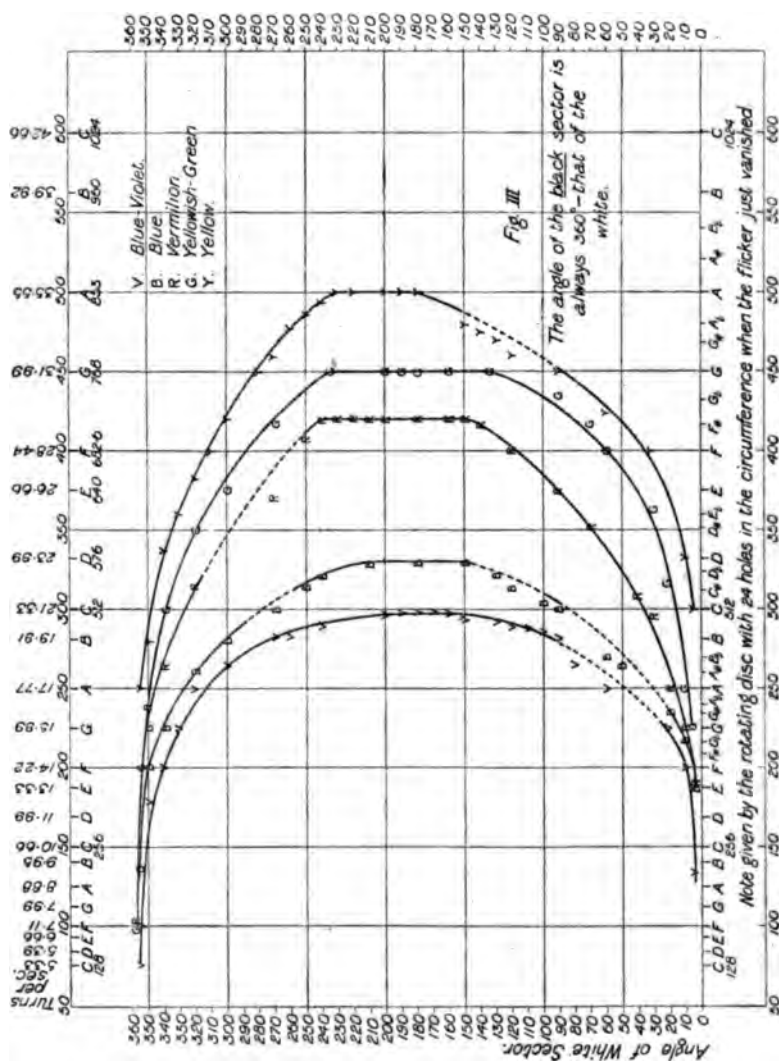
The next point seems to the writer very important, especially with reference to what follows. Numerous experiments were made to find out how the "last" (undiminished) for any one colour varied with the intensity of the light; for the present it is sufficient to say that in every case the more intense the illumination the more rapid must the rotation of the disc be made before flicker will vanish. Hence we are bound to infer that *as the stimulus applied to the retina increases in intensity, the impression produced retains its maximum value for a shorter and shorter time.*

That a brighter illumination does produce a greater stimulus (*i.e.*, that neither the contraction of the pupil, nor any other cause, overcomes the effect of brighter illumination) is conclusively proved by the fact that the brighter the light the brighter on the whole is the disc when flicker has just vanished.

Research was next made to find out in what way the rotation of a disc must be varied for flicker to vanish when the proportion of the coloured to the black sector varied, the intensity of the illumination remaining constant.

Two discs were taken: one painted with Indian ink laid on as dry as possible, and the other of the whitest cardboard procurable; these were dovetailed after Maxwell's method and mounted on a motor with a "siren" disc. This last was pierced with six, twelve, or twenty-four holes, that the note emitted might lie within the octaves which are both easy to sing and in which intervals can be most correctly judged.

As the experiments took many days and nights, and it was most important that the conditions should be as similar as possible on different occasions, the whole of the apparatus remained unmoved throughout the whole set of comparative experiments; the lime-light burner was firmly screwed down to the base-board of the lantern. The rotating disc was most carefully screened from extraneous light, though if the writer repeated the experiments he would wear a black mask to prevent the illumination of the dark sector of the disc by even the faint light reflected from the observer's face. The writer thinks this may account partly, though not wholly, for the asymmetry of the curves of fig. 3 in the case of the red and yellow. The grating was surrounded by a screen coated with the dullest optical black, whilst another screen allowed only the special colour required to fall on the half of the disc viewed. Each observer regarded the



disc from practically the same stand-point and distance, the latter being that of most distinct vision; moreover, he kept his eyes fixed on the same part of the disc, so that the image of the disc did not travel over the retina. All these are important details as will appear later.

Before examining the curves in fig. 3, consider a disc rotating under a fixed degree of illumination, *i.e.*, in a light of constant intensity. Suppose that the light is of a certain colour, *e.g.*, yellow, and that a

certain sector only, reflects the yellow light to the eye, the rest of the disc being of a perfect black. Let the disc be rotating very slowly: then, as the yellow sector passes before the eye, it will appear as bright as possible under the given illumination, i.e., it will appear of no less bright a yellow than it would, if the whole disc were yellow, i.e., the stimulus given to the part of the retina on which its image rests, is the maximum possible to this yellow, under this particular and constant illumination. Now, suppose the rate of rotation to be raised until flicker has just vanished, no part of the disc now looks so bright as the sector did at first, and since the final brightness of the disc varies directly as the width of the yellow sector (this is the fundamental assumption on which all the colour equations rest, and is verified by experiments described later), then, *when the flicker has vanished, the effective stimulus at any point of the retina is to the original and maximum stimulus as the angle of the yellow sector is to the angle of the whole disc, i.e., 360° , the illumination being constant throughout.*

Moreover, since the mere increase in the rate of rotation has no effect whatever on the *real* width and brightness of the yellow sector, but only diminishes the time during which its stimulus is applied to the retina, and diminishes, *in the same ratio*, the time the black sector takes to pass (and the more rapid passage of the black sector must, considered *alone*, tend to *increase* the brightness of the disc), whilst it (the increase in rate of rotation) *increases* to the same extent the number of stimuli applied to the retina per second, and the number of transits of the black sector; it follows, since the final apparent brightness of the disc is less than if it were *all* yellow, that *the yellow sector requires a finite time in order to produce its maximum effect, and the same argument applies to any colour.* This conclusion is in complete accordance with the results of other experimentalists.

But this is not all that these considerations prove: for since the increase in speed of rotation diminishes *in the same ratio* both the time the image of the yellow sector takes to pass over a point on the retina, and also the time the image of the black sector takes to pass (i.e., the time the sensation evoked by the yellow sector must necessarily last undiminished, if there is to be no flicker), the increased speed would have no effect whatever on the *flicker* except to multiply the number of times it occurred per second, if it were not that a weaker stimulus has a longer "last" (using the word "last" to mean the duration of the sensation undiminished, after the stimulus has been withdrawn). This is a second proof of the principle established in a different way earlier in the present paper (p. 351).

Experiments were next made to measure directly the apparent brightness of rotating *flickerless* discs, and to find an expression for

the effect of successive equal increments to the bright sector. The method used was to measure the distances from a movable source of light, of the rotating disc, and of a fixed disc, of the same colour as the bright sector of the rotating disc, when the brightness of the two discs appeared equal. It was found that for illuminations about the same as those used in the other experiments recorded in this paper, the law which connects the apparent brightness with the width of the bright sector is that enunciated before, *i.e.*, that a flickerless half-and-half disc appears half as bright as the fixed and wholly white or coloured disc, at any rate within the errors of experiment which, however, in this part of the research were not inconsiderable. When the width of the white or coloured sector was increased in steps of 10° at a time, the *increment* of the apparent brightness in the flickerless disc followed, within the errors of experiment, the series $1/0, 1/1, 1/2, 1/3, 1/4, \&c.$, as it should; and since these fractions express the *ratio* between the increase of stimulus caused at any stage by an additional 10° of coloured sector, and the stimulus already existing before its addition, it follows that we can, with the help of the principles established already, predict how the rate of rotation for the disappearance of flicker must vary with the growth of the white or coloured sector. We should expect the first few additions of the 10° to produce the most marked alteration (a rise, as we already know) in the rate of rotation necessary for the disappearance of flicker, since the diminution in the amount of the black sector is trifling in comparison with its total width. Towards the *final* additions of 10° , we shall reach a stage when the effect of the increment of 10° of white or colour is almost negligible in comparison with the total width of the white or coloured sector, but just at this time, the *relative* diminution of the black sector will be most rapidly *increasing*, and in order that flicker may only just be invisible, the rate of rotation must be considerably diminished, and this diminution will bear to the total velocity almost the ratio the diminution of the black sector (10°) bears to its total width; but not *exactly* this ratio, for since the effective stimulus is still increasing, though the increase is small compared with its total magnitude, and since this implies—apart from any effect of change in the width of the black sector—that the rate of rotation must be raised for the flicker to vanish, it follows that the rate of rotation will not diminish at so rapid a pace as the shrinkage of the black sector's width would demand, if it alone had to be considered. So far, therefore, the "flicker" curve (for a disc with a growing white or coloured sector, the angle of the bright sector being measured on the axis of Y, and the speed of rotation on the axis of X) will be, on the whole, symmetrical with respect to the straight line passing through the point on the axis of Y correspond-

ing to the half-and-half disc, but not completely so, for the consideration just mentioned would cause the curves which are represented in fig. 3 to be steeper on the right than on the left, viewing the curves from the axis of Y. It will be seen at once that in practice the converse is true, and the writer believes that this is due to the fact that the black sector is not completely black. The effect of the small percentage of light reflected from the black sector will be at first to diminish the rate of growth of the speed necessary for flicker to vanish, for it diminishes the contrast between the coloured sector and the black—a contrast on which the flicker primarily depends. The curves will therefore rise more gradually on the right, holding the figure as already described. When there is little of the black left, there will be proportionately little of the light reflected from it,—and if this light be bright enough to have any appreciable effect, the effect must be to make the decrease of the speed of rotation (necessary to cause the flicker to all but reappear) more rapid, because it lessens the effect of the narrow black sector left, giving the impression that all the coloured sector's light has survived the passage of the black sector, for a rate of rotation at which, in reality, a part failed to survive, and would have produced flicker if unaided by the light reflected from the black sector.

Thus any want of blackness in the black sector will have considerably more effect during the early part of the growth of the coloured sector, whilst the black sector is very large, than afterwards, and this completely explains the observed departure from symmetry in the curves constructed from actual observation. It should be noted that from the way in which the disc is illuminated by the spectrum, any light reflected from the black sector is of the same colour as that of the bright sector. If pigment had been used to colour the bright sector, and the disc viewed in white light, white light would have been reflected from both the coloured and the black sector, and the effect of this would be very much harder to explain.

Five curves will be seen in fig. 3; they are for different colours of the same lime-light spectrum; each of the capital letters gives the exact result of an observation, carefully verified in every case. The actual number of rotations per second can be easily found for any point on the curves by dividing the number of vibrations of the corresponding note found on the axis of X by 24, the number of holes in the syren disc. The musical intervals, so far as the diatonic scale is concerned, are the true chromatic, and not the equal temperament system.

The information conveyed by the position of any point on one of the curves may be stated as follows, taking, for example, G on the 160° line of the yellowish-green :—

The excitation of the retina caused by the stimulus of the yellowish-green light of the lime-light spectrum, reflected from "white" cardboard in $1/72$ sec. (*i.e.*, $160/360$ of $1/32$) lasts undiminished for $5/288$ sec. (*i.e.*, $200/360$ of $1/32$), *i.e.*, about $1/58$ sec.

Taking the next point G (180°) above this last point, we find that the "last" of the stimulus of the same yellowish-green, applied for $1/64$ sec. is $1/64$ sec. Thus, tabulating the results for a few more points including the above two,

Stimulus for Y.G. applied $1/72$ sec. lasts $5/288$; & $288/5 \times 72/1 = 4147$

„	„	$1/64$	„	$1/64$	„	64×64	$= 4096$
„	„	$1/58$	„	$1/72$	„	58×72	$= 4176$
„	„	$1/49$	„	$1/92$	„	49×92	$= 4508$
„	„	$1/40$	„	$1/120$	„	40×120	$= 4800$
„	„	$1/32$	„	$1/160$	„	32×160	$= 5120$

Hence the duration of the impression on the retina *undiminished* appears to decrease as the time of stimulation increases, though within narrow limits of variation one of these quantities is nearly inversely proportional to the other.

With regard to the *total* duration of a luminous impression, the writer would point out that nothing has been said in this paper; the few experiments he has made to measure this, lead to the belief that it is almost of a different order of magnitude from the time during which an impression remains undiminished, and is to be measured by whole minutes rather than by small fractions of a second.

"On the Kathode Fall of Potential in Gases." By J. W. CAPSTICK, M.A., D.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 17,—Read May 26, 1898.

It has been shown by Hittorf* that when an electric current passes through a tube containing a gas at a pressure of a few millimetres, there is a rapid fall of potential near each of the electrodes, with a much more gentle fall in the space between, and whilst the fall near the anode and in the positive column varies with the density of the gas and the current strength, the fall near the kathode is constant. Warburg† has made careful experiments on the kathode fall, and has fully established its constancy. If the gas is pure and dry, the electrodes clean, and of a metal not acted on chemically by the gas,

* 'Wied. Ann.,' vol. 20, p. 705.

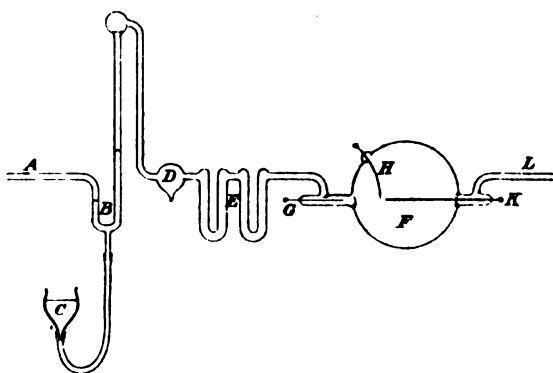
† 'Wied. Ann.,' vol. 31, p. 545; vol. 40, p. 1.

and the current not so strong as to make the negative glow cover the whole kathode or extend to the walls of the tube, the kathode fall has a definite value for each gas—a value that is independent of the pressure of the gas, or of the current strength, and that appears, in fact, to be a constant of the gas.

This being the case, it is probable that the kathode fall will prove to be connected with other physical and chemical constants of the gas, and the aim of the experiments described below was to find whether there is any intelligible relation amongst the kathode falls of three gases, one of which is formed by the combination of the other two.

The choice of suitable gases is very limited, for there must be no deposition of solid matter by the current, and no chemical action between the gas and the electrode, so that organic gases and gases containing a halogen cannot be used—at least with metal electrodes. The present investigation has been confined to water vapour, ammonia, and nitric oxide and their constituents.

The general plan of the apparatus was the same for all the gases, and is shown in the figure below.



The gas generating apparatus was sealed on at A, and a mercury pump at L. In order to isolate the generating flask from the rest, the mercury trap B was used. By raising the reservoir C, the mercury could be made to rise above the bend at B, and thus everything to the right could be exhausted. D is a small bulb to catch stray mercury. E is the purifying apparatus, and F the vessel in which the discharge took place. This vessel consisted of a globe 15 cm. in diameter, into which were sealed three wires; G, the anode of aluminium, 2 mm. thick; K, the kathode of platinum, 2 mm. thick, and extending 10 cm. into the globe, so as to afford plenty of free space for the negative glow; and H, a thin platinum wire, covered with glass to within a millimetre of the tip.

The parts of the apparatus were sealed together without glass taps or india-rubber connections.

The difference of potential to be observed is that between H and K when the current is passing between G and K. There is no need for accurate adjustment of the distance between H and K, for in the dark space the potential gradient is very slight for two or three centimetres, so that all that is required is that the end of H should be outside the negative glow. In the present experiments it was about a centimetre from the end of the kathode.

In the earlier experiments the difference of potential was measured by a bifilar quadrant electrometer, whose constant was determined by means of a battery of Clark cells. In the later experiments a Kelvin multicellular voltmeter was used.

The current through the discharge tube was supplied from 600 storage cells, a lead pencil line of variable length, drawn on a slate, being included in the circuit, to vary the current strength.

The current is not always continuous, but sometimes consists of a rapid succession of discharges, and when this is the case the observed kathode fall is not generally constant. Hence a telephone was inserted in the circuit to show when the current was continuous. With the elementary gases the telephone was generally silent, but with the compound gases the humming was so persistent as to come near wrecking the work.

Hydrogen.—Warburg found the kathode fall in hydrogen to be 300 volts, but it seemed desirable to repeat the experiments, in order to find the degree of concordance that can be obtained by different observers.

The gas was obtained from palladium that had been saturated with hydrogen prepared by the electrolysis of dilute sulphuric acid. It was purified by solid potash and phosphoric anhydride, and the discharge tube contained a piece of sodium, to destroy the last traces of moisture. The apparatus was repeatedly exhausted, heated, and refilled with hydrogen, whilst the electrodes were kept red hot by a strong current, to expel occluded gases.

The strength of the current was not measured during the experiments, but it was varied by altering the resistance in the circuit, so as to cover a varying length of the kathode with the negative glow, and so to show whether the tube was large enough to allow the discharge to pass without hindrance. The table below gives details of the measurements.

The first column gives the pressure of the gas in millimetres of mercury, the second gives the fraction of the kathode that was covered with the glow, and the third gives the kathode fall in volts.

<i>p.</i>	Glow.	E.
6	$\frac{1}{2}$	298
Apparatus exhausted and refilled.		
5	$\frac{2}{3}$	304
Apparatus exhausted and refilled.		
2	$\frac{4}{5}$	298
2	$\frac{1}{2}$	296
2	$\frac{1}{3}$	298
More gas admitted without exhausting.		
4	$\frac{4}{5}$	297
4	$\frac{1}{3}$	296

The mean result is 298 volts, which agrees so closely with the value 300 found by Warburg, that no further experiments were made.

Oxygen.—The gas was made by heating permanganate of potash, and passed through a set of Geissler bulbs of sulphuric acid and over lumps of potash and phosphoric anhydride before reaching the discharge tube. The permanganate was twice recrystallised, dried, and heated till it fell to a fine powder, before being introduced into the apparatus, in order to ensure the absence of moisture, and so diminish the risk of formation of volatile manganese compounds.

The degree of purity of the gas was tested by the spectrum of the discharge, and it proved a difficult matter to get rid of the nitrogen. By repeatedly exhausting and filling the apparatus with oxygen while the glass was kept hot, the nitrogen lines were rendered very faint, but they were not entirely removed even after continuing the operations for a fortnight. The hydrogen lines soon disappeared entirely, but the nitrogen lines were always faintly visible when the current was strong enough to make the kathode red hot. As, however, a very small amount of nitrogen is sufficient to make the nitrogen lines far brighter than the oxygen lines, the quantity of nitrogen present must have been extremely small.

In the observations recorded below, from two to six readings with different current strengths were taken at each pressure. The mean result is given for each set.

<i>p.</i>	E
11	370
7	371
4	363
$2\frac{1}{2}$	370
$1\frac{1}{2}$	373 Mean, 369

The tube was then heated, exhausted, and refilled several times, and a similar set of readings taken.

<i>p.</i>	<i>E.</i>
12	370
$9\frac{1}{2}$	372
7	368
$4\frac{1}{2}$	360
$3\frac{1}{2}$	364
3	374
$2\frac{1}{2}$	374
2	374
$1\frac{1}{2}$	373
1	373 Mean, 370

The mean of the whole is 370 volts.

Nitrogen.—Warburg's determination of the kathode fall in this gas was made on atmospheric nitrogen containing argon, and hence it was necessary to repeat the observations on chemically prepared nitrogen.

In the first experiments the gas was prepared by heating ammonium bichromate. If the bichromate is mixed with fine sand the decomposition is easily controlled, but the gas is impure, and must be passed over ignited copper and copper oxide, which involves the use of hard glass and india-rubber connections. The joints were very carefully made with thick-walled soft rubber tube, yet the observed kathode fall, 355 volts, was the same as in air, and nearly the same as in oxygen, and very much higher than what later experiments showed was the true value for nitrogen. The result is interesting, as showing the great effect of a small quantity of oxygen in the nitrogen, and emphasises the necessity for scrupulous care in removing traces of impurity.

The final experiments were made on gas prepared by the decomposition of ammonium nitrite. A solution of pure ammonium chloride was contained in a flask from the neck of which a tube a yard long passed downwards into a mercury reservoir. When the flask was exhausted along with the rest of the apparatus, potassium nitrite solution could be sucked in as required by pouring a little on the top of the mercury, and lowering the reservoir. At low pressures the mixture of ammonium chloride and potassium nitrite does not require heating, as the nitrogen comes off regularly and very slowly at the temperature of the room.

The gas was passed over solid potash and phosphoric anhydride, and a piece of sodium was placed in the discharge tube.

In some of the experiments bichromate of potash was also added to oxidise to nitric acid any oxides of nitrogen that might be formed. The addition made the evolution of gas inconveniently rapid, and the flask had to be kept cool by a water bath. The

difference between the measurements before and after the addition was very small.

It is not necessary to give a detailed list of all the observations that were made. It will be sufficient to say that before the bichromate was added twenty-five observations were made, the mean value found for the kathode fall was 231 volts, the extreme values being 223 and 241. After the addition of the bichromate twenty measurements were made, the mean value being 233 and the range 225 to 238. The mean of the whole is 232.

This is exactly the same as Warburg found for atmospheric nitrogen, whence it appears that the presence of argon has no effect on the kathode fall.

Water Vapour.—Distilled water from a clean silver still was boiled to remove dissolved gases, and the apparatus was sealed up whilst the water was still at the boiling temperature.

The greater part of the air was removed from the apparatus by means of the pump, which contained some sulphuric acid above the mercury, and the whole was then allowed to stand for a week, the acid absorbing the vapour and keeping up a steady evaporation of the water, thus gradually sweeping out any remaining air. The uncondensed gas was ejected from the pump, and the acid renewed from time to time. Incondensable gas never entirely ceased to pass over, but before any experiments were made it appeared from an estimate of the volume of water absorbed by the acid and the volume of the air bubble in the pump, that the ratio of the pressure of the air to the pressure of water vapour in the apparatus was reduced to about one part in five millions. Of course, more gas came over when the discharge was passing, but even then the quantity was very small. Presumably the hydrogen and oxygen set free near the electrodes recombine at other parts of the tube.

A compound gas naturally presents difficulties that are absent in the case of an elementary gas, for as soon as the current is started the gas becomes mixed with decomposition products, and is no longer pure. The chief difficulty, however, arose from the intermittence of the current. With elementary gases the current was seldom intermittent, with the compound gases it was seldom constant. It was, of course, useless to take any readings of the kathode fall when the telephone was singing, and many months were spent in a fruitless attempt to find what circumstances determine the constancy or intermittence of the current.

The rate of intermittence was not usually constant, as was evident from the variation in the note given by the telephone, and the change from sound to silence was sudden, and often accompanied by other changes, such as the appearance of striæ in the tube, and a sudden change in the kathode fall. When the telephone was silent

it could always in the case of the compound gases be made to sing by a sufficient increase in the current, but a reduction of the current density by the use of electrodes half an inch in diameter made it no easier to obtain a constant current.

The method by which it was hoped that errors due to the decomposition of the gas would be got rid of was to start the current, and when by accident as it proved rather than by design, the telephone was silent, to open the mercury trap and allow a stream of pure gas to play on the electrodes, whilst the reading of the electrometer was taken. It was not often that this could be carried out, for when after repeated attempts the telephone was silenced, the usual result of admitting fresh gas was either to make the current intermittent or to stop it altogether.

The proceeding was carried out successfully only twice. In the first experiment the current was steady as soon as the circuit was closed. The negative glow covered half the kathode and the kathode fall was 471 volts, but quickly rose to 484 as decomposition proceeded. A little vapour was then admitted, and the fall of potential at once sank to 467 but soon rose again to 482 when the stream of gas ceased. More vapour was then admitted whereupon the kathode fall sank to 469, and in half a minute rose again to 478.

The second experiment was less satisfactory as only a single reading was obtained. After the current had been running for a few minutes the telephone became silent, the negative glow consisting of a bright tip covering about a tenth of the kathode and the kathode fall being 484. On admitting more vapour it sank to 469, but before it had risen more than a few volts the telephone began singing again, and could not be made to stop.

Thus we have four observations of the kathode fall in the undecomposed gas, namely: 471, 467, 469, and 469, the mean being 469.

In both experiments the pressure of the vapour was about 2 mm.

Ammonia.—The gas was prepared by the action of soda on ammonium sulphate that had been treated with nitric acid in the ordinary way to remove organic substances. The ammonia was dried with lime and absorbed in calcium chloride contained in a bulb sealed to the apparatus.

Here, as with water vapour, the numerous attempts to secure a constant current met with little success. Only two readings of the kathode fall were obtained, and these were not very concordant. In the first experiment the current had been running for half an hour when the intermittence ceased, and the kathode fall in the partially decomposed gas was 510 volts. When a stream of pure ammonia was allowed to play on the kathode, the reading of the electrometer rose to 595, where it remained steady until, after a few seconds, the dis-

charge stopped. In the second experiment the current had been running for some time through gas at a low pressure, the kathode fall being 440 volts. A stream of pure gas sent it up to 570, but the telephone soon began singing, and the kathode fall sank rapidly to 480. The mean of the two observations is 582 volts.

Nitric Oxide.—A mixture of nitre and ferrous sulphate was acted on by dilute sulphuric acid, and the evolved gas washed with potash solution and sulphuric acid, and absorbed in a saturated solution of ferrous sulphate. This solution was contained in a flask, sealed to the rest of the apparatus. The gas was given off readily without warming when the pressure was sufficiently reduced, and was passed over potash and phosphoric anhydride before reaching the discharge tube.

In the case of water vapour and ammonia, when the current was discontinuous the observed kathode fall was very variable and not independent of the current strength, but with nitric oxide the variation was within narrow limits, the readings always lying between 340 and 380 volts, whether the telephone was silent or not, and whatever the current strength, provided the kathode was not covered with the negative glow.

The readings of the kathode fall and the appearance of the discharge showed that the gas is rapidly decomposed by the current. When the discharge first started, the kathode fall was always near 370 volts, and the glow at both anode and kathode was white. In a few seconds the glow round the kathode began to grow blue, and that round the anode turned pink, whilst the kathode fall slowly sank to about 345 volts. Meanwhile the glow spread backwards along the kathode, showing an increase of current, and hence a decrease in the resistance of the gas.

The decomposition proceeded so rapidly that it was impossible to get the kathode fall for the pure gas by taking the reading whilst a stream of gas played on the electrodes, for the strength of stream necessary to maintain the white glow of the pure gas was so great that the pressure immediately rose high enough to stop the discharge. Hence the only feasible plan was to allow the gas to stream through the tube long enough to sweep out the products of decomposition, then stop the stream by closing the mercury trap, pump down to a suitable pressure, start the discharge, and take a reading as quickly as possible.

Twelve readings were taken in this way, when the telephone was either silent throughout or became silent a few seconds after the current started. There was never any change in the kathode fall at the moment when the current became constant. The readings varied between 366 and 378, the mean value being 373.

This is so nearly the value for oxygen as to suggest a doubt of its

accuracy. The earlier experiments on nitrogen showed that a very slight trace of oxygen was sufficient to raise the kathode fall from its true value 232 to 355. In fact, the values for oxygen, nitric oxide, air, and nitrogen with a trace of oxygen, are all nearly the same, which makes it not improbable that in each case the oxygen alone acts as the carrier of the current.

We have, then, finally the following values for the kathode fall—

Hydrogen	298
Nitrogen	232
Oxygen	369
Water vapour	469
Ammonia	582
(Nitric oxide	373)

The last is enclosed in brackets in consequence of the doubts as to its accuracy. If we leave the result for this last gas out of account, it appears that the kathode fall is approximately an additive quantity. Ascribing the values 149, 116, and 184 respectively to the atoms of hydrogen, nitrogen, and oxygen, we get, by addition, 482 for water vapour and 563 for ammonia. As each of these depends on three measurements, they may be taken as agreeing with the observed values within the limits of experimental error. Hence, so far as the evidence of these experiments goes, the kathode fall is a property of the atoms rather than of the molecule.

As the kathode fall is constant for all pressures and currents whilst the potential gradient along the rest of the tube is variable, we may infer that no potential difference less than the kathode fall is capable of causing a discharge through the gas. This conclusion is consistent with the experiments of Mr. Peace,* who found that the minimum difference of potential that gives a discharge in air is something over 300 volts.

Assuming that the conduction is electrolytic, it seems likely, from the analogy of the electrolysis of liquids, that the kathode fall may prove to be a measure of the energy required to dissociate the gas into the ions that carry the electricity, and the present experiments were undertaken in the hope of finding some confirmation of this hypothesis. They have not, however, provided the kind of evidence that was anticipated. The results can only be reconciled with the hypothesis if further assumptions are made that would put the conduction in gases on a very different footing from the electrolytic conduction of liquids. The additive nature of the kathode fall might, for instance, be taken as an indication that the carriers of the current are provided by the disintegration of the atoms into much

* 'Roy. Soc. Proc.', vol. 52, p. 99.

smaller particles, as has already been suggested by J. J. Thomson from entirely different evidence; but the results are too few to make further speculation on their meaning of much value.

“Note on the Complete Scheme of Electrodynamic Equations of a Moving Material Medium, and on Electrostriction.”

By JOSEPH LARMOR, F.R.S., Fellow of St. John's College, Cambridge. Received May 17,—Read May 26, 1898.

This note forms a supplement to my third memoir on the “Dynamical Theory of the *Æther*,”* to the sections of which the references are made.

1. It is intended in the first place to express with full generality the electrodynamic equations of a material medium moving in any manner, thus completing the scheme which has been already developed subject to simplifying restrictions in the memoirs referred to. To obtain a definite and consistent theoretical basis it was necessary to contemplate the material system as made up of discrete molecules, involving in their constitutions orbital systems of electrons, and moving through the practically stagnant *æther*. It is unnecessary, for the mere development of the equations, to form any notion of how such translation across the *æther* can be intelligibly conceived: but, inasmuch as its strangeness, when viewed in the light of motion of bodies through a material medium and the disturbance of the medium thereby produced, has often led to a feeling of its impossibility, and to an attitude of agnosticism with reference to *æthereal* constitution, it seems desirable that a kinematic scheme such as was there explained, depending on the conception of a rotationally elastic *æther*, should have a place in the foundations of *æther*-theory. Any hesitation, resting on *à priori* scruples, in accepting as a working basis such a rotational scheme, seems to be no more warranted than would be a diffidence in assuming the atmosphere to be a continuous elastic medium in treating of the theory of sound. It is known that the origin of the elasticity of the atmosphere is something wholly different from the primitive notion of statical spring, being in fact the abrupt collisions of molecules: in the same way the rotational quality of the incompressible *æther*, which forms a sufficient picture of its effective constitution, may have its origin in something more fundamental that has not yet even been conceived. But in each case what is important for immediate practical purposes is a condensed and definite basis from which to develop the interlacing ramifications of a physical scheme: and in each case this is obtained by the use of a representation which a deeper knowledge may after-

* ‘Phil. Trans.,’ A (1897).

wards expand, transform, and even modify in detail. Although, however, it is possible that we may thus be able ultimately to probe deeper into the problem of æthereal constitution, just as the kinetic theory has done in the case of atmospheric constitution, yet there does not seem to be at present any indication whatever of any faculty which can bring that medium so near to us in detail as our senses bring the phenomena of matter: so that from this standpoint there is much to be said in favour of definitely regarding the scheme of a continuous rotationally elastic æther as an ultimate one.

A formal scheme of the dynamical relations of free æther being postulated after the manner of Maxwell and MacCullagh, and a notion as clear as possible obtained of the æthereal constitution of a molecule and its associated revolving electrons, by aid of the rotational hypothesis, it remains to effect with complete generality the transition between a molecular theory of the æthereal or electric field which considers the molecules separately, and a continuous theory expressed by differential equations which take cognizance only of the properties of the element of volume, the latter alone being the proper domain of mechanical as distinct from molecular theory. This transformation is, as usual, accomplished by replacing summations over the distribution of molecules by continuous integrations over the space occupied by them. In cases where the integrals concerned all remain finite when the origin to which they refer is inside the matter so that the lower limit of the radius vector is null, there is no difficulty in the transition: this is for example the case with the ordinary theory of gravitational forces. But in important branches of the electric theory of polarised media, some of the integral expressions become infinite under these circumstances; and this is an indication that it is not legitimate to replace the effect of the part of the discrete distribution of molecules which is adjacent to the point considered by that of a continuous material distribution. The result of the integration still, however, gives a valid estimate of the effect of the material system *as a whole*, if we bear in mind that the infinite term coming in at the inner limit really represents a finite part of the result depending *solely* on the local molecular configuration, a part whose actual magnitude could be determined only when that configuration is exactly assigned or known. The consideration of this indeterminate part is altogether evaded by means of a general mechanical principle which I have called the principle of mutual compensation of molecular forcives. This asserts that in such cases, when a finite portion of the effect on a molecule arises from the action of the neighbouring molecules, this part must be omitted from the account in estimating the *mechanical* effect on an element of volume of the medium; indeed otherwise mechanical *theory would be impossible*. The mutual, statically equilibrating,

actions of *adjacent* molecules determine the structure of the medium, and any change therein involves change in its *local* physical constants and properties, which may or may not be important according to circumstances: but such local action contributes nothing towards polarising or straining the element of mass whose structure is thus constituted, and therefore nothing to mechanical excitation, unless at a place where there is abrupt change of density.* In the memoir above mentioned this molecular principle was applied mainly to determine the mechanical stress in a polarised material medium. It necessarily also enters into the determination of the electrodynamic equations of a moving medium treated as a continuous system, and even of a magnetised medium at rest, from consideration of its molecular constitution. It is here intended only to record in precise form the general scheme that results from it, details of demonstration being for the present reserved. Everything being expressed in a continuous scheme per unit volume, let (u, v, w) denote the current of conduction, (u, v, w) the total current of Maxwell, (f, g, h) the electric displacement in the æther and (f', g', h') the electric polarisation of the molecules so that the total so-called displacement flux of Maxwell is $(f+f', g+g', h+h')$; let ρ be the volume-density of uncompensated electrons or the density of free charge, let (A, B, C) be the magnetisation, and (p, q, r) the velocity of the matter with respect to the stagnant æther. As before explained (§ 13, footnote), the convection of the material polarisation (f', g', h') produces a *quasi*-magnetisation $(rg'-qh', ph'-rf', qf'-pg')$ which adds on to (A, B, C) . Also, as before shown, the vector potential of the æthereal field, so far as it comes from the molecular electric whirls which constitute magnetisation, is given, for a point outside the magnetism, by

$$\begin{aligned} F &= \iint \left(B \frac{d}{dz} - C \frac{d}{dy} \right) \frac{1}{r} d\tau \\ &= \int (Bn - Cm) r^{-1} dS + \int \left(\frac{dC}{dy} - \frac{dB}{dz} \right) \frac{1}{r} d\tau, \end{aligned}$$

(lmn) being the direction vector of dS , and therefore is that due to a bodily current system $\left(\frac{dC}{dy} - \frac{dB}{dz}, \dots, \dots \right)$ together with current sheets on the interfaces. When the point is inside the magnetism, there are still no infinities in the integral expressing F , and this transformation of it by partial integration is still legitimate. But

* This exception explains why the mechanical tractions on an interface, determined in § 36 as the limit of a gradual transition, are different from the forces on the Poisson equivalent interfacial distribution.

the spacial differential coefficients of (F, G, H) are also involved in the forcives of the æthereal field, and with them the case is different: the transformation by parts is then analytically wrong, owing to neglect of the infinite elements at the origin, while in actuality a finite portion of the whole effect arises from the influence of the neighbouring molecules. We have, therefore, by the molecular principle, to separate the infinite elements from the integrals and leave them out of account; and this is effected by employing the second form above for F, which differs from the first form only in having got rid of the local terms at the origin in its differential coefficients. Thus it is not merely convenient, but even necessary for a mechanical theory, which considers distributions instead of individual molecules, to replace magnetism by its equivalent continuous current system as here. The *quasi*-magnetism arising from electric convection adds to this equivalent current system the additional bodily terms

$$\left\{ \frac{d}{dy} (qf' - pg') - \frac{d}{dz} (ph' - rf'), \dots, \dots \right\}$$

together with surface sheets: thus the volume current so added has for *x*-component

$$\frac{\partial f'}{\partial t} - \frac{df'}{dt} - p \left(\frac{df'}{dx} + \frac{dg'}{dy} + \frac{dh'}{dz} \right) - f' \frac{dp}{dx} - g' \frac{dp}{dy} - h' \frac{dp}{dz},$$

where $\frac{\partial f}{\partial t}$ represents $\frac{df'}{dt} + \frac{dpf'}{dx} + \frac{dqf'}{dy} + \frac{drf'}{dz}$, or the rate of change of *f'* supposed associated with the moving matter. Combining all these parts, the current and magnetism together are completely represented as regards determination of electric effect by what we may call the *total effective current* (u_1, v_1, w_1) where

$$u_1 = u' + \frac{dC}{dy} - \frac{dB}{dz} + \frac{df}{dt} + \frac{\partial f'}{\partial t} - \left(f' \frac{dp}{dx} + \frac{dpq'}{dy} + \frac{dph'}{dz} \right) + p\rho,$$

together with superficial current sheets arising from the true magnetism (A, B, C) and the electric convection. Since ρ is equal to

$$\frac{d(f+f')}{dx} + \frac{d(g+g')}{dy} + \frac{d(h+h')}{dz}$$

we may write

$$u_1 = u' + \frac{dC}{dy} - \frac{dB}{dz} + \frac{df}{dt} + \frac{\partial f'}{\partial t} - \left(f' \frac{dp}{dx} + g' \frac{dp}{dy} + h' \frac{dp}{dz} \right) + p \left(\frac{df}{dx} + \frac{dy}{dy} + \frac{dh}{dz} \right)$$

in which the last term may be expressed as $-\nabla^2\psi/4\pi c^2$.

It is to be observed that this effective current satisfies the condition of incompressible flow,* which by definition (or rather by the æthereal constitution) is necessarily satisfied by the *total current* (u, v, w) of the previous memoirs; for the additional terms which represent the magnetism clearly satisfy the stream relation.† The remainder of the scheme of electrodynamic relations is established as in the previous memoirs. Thus (F, G, H) now representing simply $\int(u_1, v_1, w_1)r^{-1}d\tau$, which satisfies the stream relation $dF/dx + dG/dy + dH/dz = 0$ because (u_1, v_1, w_1) is a stream vector, we deduce an electric force (P, Q, R) acting on the electrons, where

$$P = cq - br - dF/dt - d\psi/dx,$$

also an æthereal force (P', Q', R') straining the æther, where

$$P' = (4\pi c^2)^{-1}f = -dF/dt - d\psi/dx,$$

the function ψ being determined in each problem so as to avoid æthereal compression.

Across an abrupt transition, F, G, H and the normal component of (u_1, v_1, w_1) must be continuous, thus making up the *four* necessary and sufficient interfacial conditions. The gradients of F, G, H are, however, not continuous when there is magnetisation or dielectric convection, on account of the effective interfacial current sheets before mentioned.

The exact value of the mechanical force (X, Y, Z) per unit volume, comes out as

$$X = \left(v - \frac{dy}{dt}\right)\gamma - \left(w - \frac{dz}{dt}\right)\beta + A\frac{dx}{dx} + B\frac{dx}{dy} + C\frac{dx}{dz} \\ + f'\frac{dP'}{dx} + g'\frac{dP'}{dy} + h'\frac{dP'}{dz} + \rho P',$$

where $\alpha = dH/dy - dG/dz - 4\pi A$.

* It is proposed to call a flow-vector which obeys this condition a *stream*, the more general term *flow* or *flux* including cases like the variable stage of the flow of heat in which the condition of absence of convergence is not satisfied. The two main classes of physical vectors may be called *fluxes* and *gradients*, the latter name including such entities as forces and being especially appropriate when the force is the gradient of a potential. Lord Kelvin's term *circuital flux* has previously been used to denote a *stream* vector; but it is perhaps better to extend it to a general vector which is directed along a system of complete circuits.

† The (u, v, w) of § 13, however, included a part arising from convection of electric polarisation. Notice that when this is transferred to the magnetism, as here, we have $u = n' + df/dt + df'/dt + \rho\rho$: thus when there is no conduction and ρ is therefore wholly convected so that $\delta\rho/dt$ is null, the stream character of the total current simply requires $d(f+f')/dx + d(g+g')/dy + d(h+h')/dz = \rho$, so that the formulation is now easier and more natural.

In these formulæ, with the exception of the one for (u_1, v_1, w_1) above, (A, B, C) includes the *quasi-magnetism* arising from electric convection, while (u, v, w) is the total electric current that remains after all magnetic effect of whatever type has been omitted. It is to be noted that the final terms in \mathbf{X} involve in strictness the æthereal force, instead of the electric force as in § 39.

It follows from the formula for (P, Q, R) that

$$\frac{dR}{dy} - \frac{dQ}{dz} = -\frac{\delta a}{dt} + \left(a \frac{d}{dx} + b \frac{d}{dy} + c \frac{d}{dz} \right) p;$$

hence *Faraday's circuital relation holds good provided the velocity (p, q, r) of the matter is uniform in direction and magnitude.*

Again, since (F, G, H) is a stream vector,

$$\frac{dc}{dy} - \frac{db}{dz} = -\nabla^2 F = 4\pi \left(u + \frac{dC}{dy} - \frac{dB}{dz} \right),$$

where (u, v, w) represents the total current of Maxwell, and (A, B, C) the whole of the magnetism and the *quasi-magnetism* of convection: hence

$$\frac{d\gamma}{dy} - \frac{d\beta}{dz} = 4\pi u,$$

so that *Ampère's circuital relation holds, with the above definition of (α, β, γ) , under all circumstances.*

But in circumstances of electric convection these two circuital relations would not usually by themselves form the basis of a complete scheme of equations, as they do when the material medium is at rest.

To complete the scheme, the above dynamical equations must be supplemented by the observational relations connecting the conduction current with the electric force, the electric polarisation with the electric force, and the magnetism with the magnetic force. In the simplest case of isotropy these relations are of types

$$u' = \sigma P, \quad f' = (K-1)/4\pi c^2 P, \quad A = \kappa \alpha + (r g' - q h').$$

It is to be observed that the physical constants which enter into the expression of these relations will presumably be altered by motion through the æther of the material system to which they belong: but because there is nothing unilateral in the system, a reversal of this motion should not change the constants, therefore their alteration must depend on the square of the ratio of the velocity of the system to that of radiation, and would only enter in a second approximation.

The various problems relating to electric convection and optical

aberration worked out in §§ 14—16, pp. 225—229, will be found to fit into this scheme. I take the opportunity of correcting an erratum in p. 226, lines 16, 17, which should read

$$\Psi_1 = \frac{1}{3}(1 + K^{-1})\omega c r^2 + A r^2(\cos^2 \theta - \frac{1}{3}) + A'$$

$$\Psi_2 = B r^{-3}(\cos^2 \theta - \frac{1}{3}) + B' r^{-1},$$

with of course different values of the constants.

2. In a material dielectric the bodily mechanical force is derived from a potential $-(K-1)F^2/8\pi$, and there is also a normal inward traction $KF^2/8\pi$ where it abuts on conductors. For the thin dielectric shell of a condenser this force could be balanced by a hydrostatic pressure $(K-1)F^2/8\pi$ together with a Maxwell stress consisting of a pressure $F^2/8\pi$ along the lines of force and an equal tension at right angles to them: in fact this reacting system gives the correct traction over the faces of the sheet and the correct force throughout its substance. If the sheet has an open edge the tractions on that edge are however not here attended to; when the sheet is thin these are of small amount, and their effect is usually local, as otherwise the nature of the edge would be an important element. Moreover, in the most important applications of the formula the edge is of small extent, so that they form a local statically balanced system. The stress above specified will thus represent the material elastic reaction, provided the strains in the different elements of volume, which correspond to it, can fit together without breach of continuity of the solid material. This condition will be secured if the shell is of uniform thickness so that F is constant all over it: in that case, therefore, the elastic reaction in the material will make up a pressure $KF^2/8\pi$ along the lines of force and a pressure $(K-2)F^2/8\pi$ in all directions at right angles to them, which is the result obtained for solids in § 76.

If, however, the coatings of the condenser are not supported by the dielectric shell, the elastic reaction in the shell will be simply a pressure $(K-1)F^2/8\pi$ uniform in all directions. This is what actually occurs in the case of a fluid dielectric, where such support is not mechanically possible.

It appeared from § 79 that in glass there is actually an increase of volume under electric excitation, while the mechanical forces would produce a diminution: and the same is true for most dielectric liquids, the fatty oils being exceptions,* though by a confusion between action and reaction the result was there stated as the opposite. It thus appears that in general an intrinsic expansion, in addition to the effects of the mechanical force, accompanies electric

* In the cognate case of magnetisation of ferrous sulphate solution, Hurmuzescu finds a contraction of volume.

excitation of material dielectrics. This circumstance will perhaps recall to mind Osborne Reynolds' theory of the dilatancy of granular media, which explains that the discrete elements of such media tend to settle down *under the mutual influences of their neighbours* so as to occupy the smallest volume, and therefore any disturbing cause has a tendency to increase the volume.

In § 80, on the influence of electric polarisation on ripple velocity, the result stated for dielectrics should be doubled. It is to be remarked that a horizontal dielectric liquid surface becomes unstable in a uniform vertical electric field when the square of the total continuous vertical electric displacement exceeds the moderate value $\frac{K_1 K_2 (K_2 + K_1)}{2\pi (K_2 - K_1)^2} \{(\rho_2 - \rho_1)gT\}^{\frac{1}{2}}$ electrostatic units, T being the capillary tension. For a conducting liquid instability ensues when the square of the surface electric density exceeds $\frac{K_1^2}{2\pi} \{(\rho_2 - \rho_1)gT\}^{\frac{1}{2}}$ electrostatic units. In exciting a dielectric liquid by the approach of an electrified rod it must often have been noticed that when the rod is brought too near, the liquid spurts out vigorously in extremely fine filaments or jets: the fineness of the filaments may be explained, in part at any rate, after Lord Rayleigh ('Phil. Mag.' 1882, "Theory of Sound," § 364), without assuming an escape of electricity into the liquid, as arising from the circumstance that it is only narrow crispations of the surface, and not extensive deformations, that become unstable.

The opportunity is taken to correct other errata in the memoir, 'Phil. Trans.,' A, 1897, as follows:—

page 252, line 15, *read* 8π *for* 4π .

„ 253, „ 18, the factor c^2 is omitted.

„ 297, „ 4, *dele* m .

June 9, 1898.

The Annual Meeting for the Election of Fellows was held this day.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Professor Bonney and Mr. R. H. Scott were, with the consent of the Society, nominated Scrutators to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Baker, Henry Frederick, M.A.	Preston, Professor Thomas, M.A.
Brown, Professor Ernest William.	Reid, Professor Edward Way-
Buchan, Dr. Alexander, M.A.	mouth, M.B.
Harmer, Sidney Frederic, M.A.	Scott, Alexander, M.A.
Lister, Arthur, F.L.S.	Seward, Albert Charles, M.A.
McMahon, Lieutenant - General	Shenstone, William Ashwell,
Charles Alexander.	F.I.C.
Osler, Professor William, M.D.	Taylor, Henry Martyn.
Parsons, Hon. Charles A., M.A.	Wimshurst, James.

Thanks were given to the Scrutators.

June 9, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Professor W. Haswell (elected, 1897) and Professor Amagat (elected a Foreign Member, 1897) were admitted into the Society.

The following Papers were read :—

- I. "On a New Constituent of Atmospheric Air." By Professor W. RAMSAY, F.R.S., and MORRIS W. TRAVERS.
- II. "On the Position of Argon, Helium, and Krypton in the Scheme of Elements." By Sir WILLIAM CROOKES, F.R.S.
- III. "Experimental Investigations on the Oscillations of Balances." By D. MENDELÉEFF, For. Mem. R.S.
- IV. "Experiments on Aneroid Barometers at Kew Observatory and their Discussion." By Dr. C. CHREE, F.R.S.

- V. "The Nature of the Antagonism between Toxins and Antitoxins." By Dr. C. J. MARTIN and Dr. T. CHERRY. Communicated by Dr. HALLIBURTON, F.R.S.
- VI. "Some Differences in the Behaviour of Real Fluids from that of the Mathematical Perfect Fluid." By A. MALLOCK. Communicated by Lord RAYLEIGH, F.R.S.
- VII. "On the Heat dissipated by a Platinum Surface at High Temperatures." By J. E. PETAVEL. Communicated by Lord RAYLEIGH, F.R.S.

"An Extension of Maxwell's Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and allied Phenomena." By EDWIN EDSER, A.R.C.S. Communicated by Captain W. DE W. ABNEY, C.B., F.R.S. Received February 18,—Read March 10, 1898.

The Electro-magnetic Theory of Light, as left by Maxwell, gave no explanation of dispersion, and led to conclusions in some respects inconsistent with the results of experiments on metallic reflection. There seems to be little doubt as to the general direction in which it would be necessary to modify that theory in order to give a satisfactory account of these phenomena. Electrical conduction has been considered by many to be inseparably connected with the motion of charged atoms, whilst the properties of a dielectric have been found to admit of an explanation on somewhat similar lines; consequently it would appear necessary, where fluctuations in the electric field of frequencies as great as those of light are concerned, to expressly formulate the reactions of the atoms or molecules composing the medium through which the disturbances are propagated. The mechanical theories of light, when modified in a similar manner, have been found capable of giving a more or less comprehensive account of dispersion and metallic reflection; and it would appear that the assumptions necessitated in the present case are at least as admissible as those which have been made elsewhere. No doubt a theory with any pretension to finality must include a satisfactory account of the nature of the luminiferous ether and of electricity, perhaps even of the ultimate constitution of atoms; and Mr. Larmor's investigations show how far we can even now go in this direction. On the other hand, a less comprehensive theory, depending only on the known laws of electrical actions, may prove not without value, if, whilst explaining the observed phenomena, it enables us to form a clear mental picture of the processes involved.

In what follows, I have endeavoured to extend Maxwell's theory so as to include dispersion and metallic reflection. The initial assumption that both conducting and dielectric media consist of molecules, each comprising (in the simplest case) two oppositely charged atoms, is essentially that made by Helmholtz in his paper on the "Electro-magnetic Theory of Dispersion."* The methods I have employed are however different, and my final results, though bearing a general resemblance to those obtained by Helmholtz, differ from them in some important particulars, besides being simpler and more directly related to the results of investigations on other theories of the nature of light. But besides the gain in simplicity and consequent physical definiteness, I have had other reasons for not following Helmholtz's method. The utility of employing the principle of least action in this case may be questioned, since the only difficulty appears to lie in formulating the reactions due to the charged atoms, and the nature of these reactions is implied in the energy equations assumed. Moreover, the energy equations obtained by Helmholtz are themselves open to criticism, and some of the results obtained do not reduce to Maxwell's equations when the terms involving the polarization of the dielectric are equated to zero.†

2. This paper contains (1) a simple explanation of the fundamental phenomena observed in connection with a state of steady electrical strain in a dielectric; (2) a consideration of the law of propagation of electrical disturbances in a polarizable medium; equations are obtained which explain dispersion, both ordinary and anomalous; (3) and a consideration of metallic reflection, especially in connection with Kundt's experiments on the velocity of light in metals.

State of Steady Electrical Strain in a Dielectric.

3. It is here sought to explain Faraday's discovery, that the introduction of a material dielectric between the plates of a charged condenser, diminishes the potential difference between the plates.

* H. von Helmholtz, 'Wied. Ann.,' 1893, vol. 48, pp. 389—405, 723—725. Translated by Dr. Howard in the 'Electrician,' vol. 37, pp. 404—408.

† Helmholtz's Theory of Dispersion has been criticised by Rieff ('Wied. Ann.,' 1895, vol. 55, pp. 82—94) and Heaviside ('Electrician,' vol. 37, Aug. 7, 1896). In addition it would appear that the employment of a term expressing the dissipation of energy (other than Rayleigh's "dissipation function") is inadmissible in the principle of least action (Larmor, 'Brit. Assoc. Report,' "On the Action of Magnetism on Light," 1893 (Nottingham)). The final equations obtained by Helmholtz, when the terms relating to the polarization of the medium are equated to zero, are of the nature

$$\dot{\mathbf{B}} = \text{curl } \mathbf{E} \text{ and } -4\pi\dot{\mathbf{D}} = \text{curl } \mathbf{H},$$

where \mathbf{B} , \mathbf{E} , \mathbf{D} , and \mathbf{H} represent the (vector) magnetic induction, electromotive intensity, displacement, and magnetic force respectively. They differ in sign from Maxwell's equations.

Let us consider a dielectric medium, other than the ether, to be composed of molecules each comprising, in the simplest case, two oppositely charged atoms at a definite distance apart. The volume actually contained by the atoms is provisionally assumed to be small in comparison with the volume of the interatomic spaces.

In what follows the term *axis of a molecule* will be applied to the vector distance between a negative and its associated positive atom. In an isotropic medium the axes of the various molecules will, in the absence of electrical strain, be inclined indifferently in all directions, so that any element of volume will have no resultant electric moment. If now a difference of potential be established between any two parallel planes in the medium, the positive atoms will all move toward points of lower, and the negative atoms toward points of higher potential. As a result each element of volume will now possess a resultant electric moment. Also, if we define a molecular electric moment as the product of the atomic charge (taken as positive) into the vector distance between a negative and its associated positive atom, the resultant electric moment due to a strained element of volume containing a number of molecules, will coincide in direction with the direction of fall of potential.

In the unstrained medium we may represent the several moments of the molecules contained in an element of volume by lines radiating uniformly from a point, and ending on the surface of a sphere.*

To find the electric moment of an element of the strained medium, it is only necessary to determine the sum of the alterations of the component molecular moments in the direction of the fall of potential.

4. Let P be the electromotive intensity at a point in the dielectric, and let q be the atomic charge, the distance between two associated atoms being l . Then a molecule whose axis makes an angle θ with the direction of fall of potential, will be subjected to a couple $Pql \sin \theta$ tending to decrease θ and each atom will be subjected to a force $Pq \cos \theta$ tending to increase l . Assuming that the forces of restitution called into play by the displacement of the atoms are in both cases proportional to the linear displacements, we shall have

$$\frac{l}{2} \phi = K_1 P q \sin \theta, \quad \frac{\Delta l}{2} = K_2 P q \cos \theta,$$

where ϕ indicates the (infinitesimal) molecular rotation, and $\frac{1}{2} \Delta l$ is the linear displacement of either atom in a line with the axis of the molecule.

The increase of the component molecular moment in the direction of the fall of potential due to these strains, will be

$$ql\phi \sin \theta + q\Delta l \cos \theta.$$

* Maxwell's 'Electricity and Magnetism,' vol. 2, § 443.

Substituting for ϕ and Δl this expression becomes

$$2K_1Pq^2 \sin^2\theta + 2K_2Pq^2 \cos^2\theta.$$

Also, if dv = the volume of the element of the dielectric, and if there are n molecules per unit volume, the number of molecules whose axes are inclined at angles between θ and $\theta + d\theta$ will be

$$\frac{ndv}{2} \sin \theta \cdot d\theta.$$

Hence the resultant moment of the element dv of the strained medium will be

$$\begin{aligned} & ndvPq^2 \left\{ K_1 \int_0^\pi \sin^3\theta d\theta + K_2 \int_0^\pi \cos^3\theta \sin \theta d\theta \right\} \\ &= ndvPq^2 \left(\frac{4}{3} K_1 + \frac{4}{3} K_2 \right) \\ &= PMdv \dots\dots\dots (1), \end{aligned}$$

$$\text{if} \quad M = nq^2 \left(\frac{4}{3} K_1 + \frac{4}{3} K_2 \right) \dots\dots\dots (2).$$

The electro-motive intensity, parallel to the direction of the fall of potential, due to this element of the strained medium, at a point distant r from it in a direction making an angle θ with the direction of the resultant moment, will be

$$- PMdv \frac{(1 - 3 \cos^2\theta)}{r^3}.$$

Taking the point at which we wish to determine the electromotive intensity as origin, and the axis of x parallel to the direction of fall of potential, we shall have for the electromotive intensity due to a slab of the strained dielectric, perpendicular to x and at a distance x from the origin (if dx = the thickness of the slab, and $h = r \sin \theta$)

$$- PM \cdot 2 \pi dx \int_0^\infty \left(\frac{1}{(x^2 + h^2)^{\frac{5}{2}}} - \frac{3x^2}{(x^2 + h^2)^{\frac{7}{2}}} \right) h dh = 0.$$

The only slab of the dielectric which contributes anything to the electro-motive intensity at the origin is that lying between the planes at $-\frac{1}{2}dx$ and $+\frac{1}{2}dx$. The electro-motive intensity due to this will be

$$- PM \cdot 2 \pi dx \int_0^\infty \frac{h dh}{\left(h^2 + \left(\frac{dx}{2} \right)^2 \right)^{\frac{5}{2}}} = - 4 \pi PM.$$

Hence if D be the displacement other than that produced by the polarization of the medium

$$\begin{aligned} P &= 4 \pi D - 4 \pi MP \\ \therefore P (1 + 4 \pi M) &= 4 \pi D \dots\dots\dots (3). \end{aligned}$$

Hence $(1+4\pi M)$ is the value of the specific inductive capacity measured in the electro-static system. In the electro-magnetic system (3) must be written

$$P\kappa\left(1+\frac{4\pi M}{\kappa}\right)=4\pi D \dots\dots\dots (4),$$

where κ is the dielectric constant of the ether.

5. A few remarks may be made in connection with the assumptions made in the course of the above argument. An objection might be raised to the supposition that two oppositely charged atoms could remain in equilibrium at a definite distance apart. To this it may be replied that the form of the argument does not necessitate the assumption that electrical forces are the only forces acting; and even if these were the only forces, it would be possible to account for the atomic separation by supposing both atoms to be revolving about their common centre of gravity.

If the atoms are considered to be small conducting spheres, and if the volume actually occupied by the atoms is a large fraction of the volume occupied by the dielectric, a correction on the lines of the Mosotti-Clausius dielectric theory would become necessary. On the other hand, if electrical conductivity is inseparably connected with the motion of charged atoms, it would appear to be wrong to consider the atoms themselves to be conductors.

If finite motions of the atoms were necessary to explain the properties of a dielectric, a treatment of the problem similar to that used by Weber, in his molecular theory of magnetism would be necessitated. It may, however, easily be shown that infinitesimal atomic displacements will account for the observed phenomena. Take the case of two plane and parallel charged condenser plates. Let P be the electro-motive intensity at a point between the plates when charged *in vacuo*. If now, the charge remaining the same, pure water is introduced between the plates, the electro-motive intensity will become

$$P-\frac{4\pi nq\delta}{\kappa},$$

if δ = the average molecular displacement. Taking Cohn and Arons's value for the specific inductive capacity of water, viz., 76, we have

$$\frac{P}{P-\frac{4\pi nq\delta}{\kappa}}=76.$$

Let $P = 100$ volts. Then, since nq will equal the total charge carried by the oxygen in 1 gram of water, we shall have

$$\delta = \frac{75}{76} \cdot \frac{10^{10}}{12.5 \times \frac{10^4}{9} \times 9 \times 10^{20}} = 8 \times 10^{-16}.$$

Hence the magnitude of the displacement, necessary to account for the dielectric properties of a medium possessing the highest specific inductive capacity known, is small, even when compared with molecular magnitudes.

In determining the force acting on a charged atom within a polarised dielectric, a difficulty arises somewhat similar to that experienced when the force on a magnetic molecule within a magnet is sought.* The above method, I think, is not open to very serious objection, and as the results obtained give a fair explanation of the experimental facts, it may, perhaps, be tentatively adopted.

[*Added May 10.*—Mr. Larmor† gives $P + \frac{1}{3}\pi I$ as a first approximation to the value of the total electric field at a point within a polarized dielectric, I denoting the intensity of electrification, or the electric moment of the polarized medium per unit volume. As a consequence, the Lorenz refraction equivalent is obtained, whilst the relation $(\mu^2 - 1)\infty$ density follows from the reasoning employed in the present paper. Further, a dispersion formula, differing somewhat from that of Ketteler, is obtained by Mr. Larmor; it would be interesting to know with what degree of accuracy this formula is capable of representing the dispersion of transparent substances, as determined, for instance, by Paschen and Rubens.‡

The following somewhat simple argument is submitted in justification of the assumption made above, that the field in a polarized medium is equal to P .

Let the space between two plane and parallel condenser plates, separated by a distance small in comparison with the area of either, be filled with an isotropic polarizable medium, such as that assumed above. The molecules are assumed to be in a state of equilibrium amongst themselves when the two plates are at the same potential; consequently the potential energy W , due to the small rotations $\phi_1, \phi_2, \phi_3, \dots$ of the several molecules composing the medium may be represented by a quadratic function of the form

$$W = \frac{1}{2} \{ a_1 \phi_1^2 + a_2 \phi_2^2 + \dots + 2 b_{12} \phi_1 \phi_2 + 2 b_{13} \phi_1 \phi_3 + \dots \}.$$

A similar expression would obtain for the potential energy due to the separation of the atoms within the molecules. The conditions connecting the various coefficients are such that W is essentially positive.

Considering for a moment only the molecular rotations, the force

* See Maxwell's 'Electricity,' vol. 1, p. 83, 1892 edition, footnote by Professor J. J. Thomson.

† "A Dynamical Theory of the Electric and Luminiferous Medium, Part III," 'Phil. Trans.,' A, vol. 190 (1897), § 18—21, pp. 232—236.

‡ Paschen, 'Wied. Ann.,' 1894, vol. 53, pp. 812—822; Rubens, 'Wied. Ann.,' 1895, vol. 54, pp. 476—485.

of restitution F_1 called into play by the rotation ϕ_1 will be equal to dw/dh_1 , where $h_1 = \frac{l}{2}\phi_1$. Thus

$$F_1 = \frac{2}{l} \{a_1\phi_1 + b_{12}\phi_2 + b_{13}\phi_3 + \dots\}.$$

The first term represents the force acting on an atom of the molecule whose subscript is unity, when that molecule alone is displaced; the succeeding terms represent the force on the same atom, due to the displacement of the remaining molecules throughout the medium. A number of relations, similar to those given by Maxwell,* may be deduced, but require no further notice here.

Let the conducting surfaces, each of area A , and separated by a distance D , be charged by means of a battery to a constant potential difference V . Let $\phi_1, \phi_2, \phi_3, \dots$ be the molecular rotations produced. If, when electrical equilibrium has been acquired, the molecular rotations are further increased by $d\phi_1, d\phi_2, d\phi_3, \dots$, the increment of electrical energy supplied by the battery will be equal to $A \cdot V dI$. Further, writing $V = PD$, we obtain for this energy E , the equivalent expressions

$$E = AD \cdot P dI_1 = P \Sigma l q \sin \theta d\phi,$$

where Σ indicates summation for the whole of the molecules throughout the medium. Since, on releasing the molecules, the above amount of electrical energy will be returned to the battery (the resistance of leads being considered negligible), we must have

$$P \Sigma l q \sin \theta_n d\phi_n = \Sigma 2d\phi_n \{a_n\phi_n + b_{n1}\phi_1 + b_{n2}\phi_2 + \dots\}.$$

Since the $d\phi_1$'s are arbitrary,

$$Plq \sin \theta_1 d\phi_1 = 2d\phi_1 \{a_1\phi_1 + b_{12}\phi_2 + b_{13}\phi_3 + \dots\}.$$

Hence $F_1 = Pq \sin \theta_1$. Consequently the couple acting on a polarized molecule will be equal to that due to a field P . Further, if $P = 4\pi\sigma$, it is easily seen that

$$\frac{2}{l} a_1 \phi_1 = 4\pi\sigma \cdot q \sin \theta_1.$$

Similar reasoning will apply when the inter-atomic separations are considered. Consequently the total electrical field contributed by the molecules throughout the medium is numerically equal to $4\pi I_1$, and acts in a direction opposed to P , the result formerly obtained.]

* 'Treatise,' vol. 1, chap. 3, §§ 87, 88.

Propagation of Electrical Disturbances.

6. Only a very slight modification of Maxwell's equations appears to be necessary in order to determine the law governing the propagation of electrical disturbances in a polarized medium such as that previously considered.

Let P, Q, R be the components of the electromotive intensity at a point, F, G, H being the components of the vector potential there. The electromotive intensity due to the displacement of the atoms may, if the velocity of the atoms is small compared with the velocity of light, be derived from a potential ψ .

$$\text{Hence} \quad \left. \begin{aligned} P &= -\frac{dF}{dt} - \frac{d\psi}{dx} \\ Q &= -\frac{dG}{dt} - \frac{d\psi}{dy} \\ R &= -\frac{dH}{dt} - \frac{d\psi}{dz} \end{aligned} \right\} \dots\dots\dots (5).$$

Here it must be remembered that ψ will be a function of t .

We may define the vector potential so that

$$\frac{\partial F}{\partial x} + \frac{\partial G}{\partial y} + \frac{\partial H}{\partial z} = 0.$$

In that case

$$\frac{\partial P}{\partial x} + \frac{\partial Q}{\partial y} + \frac{\partial R}{\partial z} = -\nabla^2 \psi = 0 \dots\dots\dots (6),$$

since any element of volume, if taken so as to comprise a sufficient number of molecules, will contain as many positively charged as negatively charged atoms.

Let a, b, c represent the components of magnetic induction.

Then

$$a = \frac{dH}{dy} - \frac{dG}{dz},$$

with similar equations for b and c .

Hence, from (5),

$$\left. \begin{aligned} \frac{da}{dt} &= \frac{dQ}{dx} - \frac{dR}{dy} \\ \frac{db}{dt} &= \frac{dR}{dx} - \frac{dP}{dz} \\ \frac{dc}{dt} &= \frac{dP}{dy} - \frac{dQ}{dz} \end{aligned} \right\} \dots\dots\dots (7).$$

These equations are identical with those used by Professor J. J. Thomson in his developments of Maxwell's theory.* They express the law that however the electromotive intensity at any place may be modified by the presence of the charged atoms, its line integral round a closed curve will be equal to the rate of decrease of the induction through the curve.

These equations differ from those obtained by Helmholtz.†

7. If α , β , γ are the components of the magnetic force, and u , v , w those of the electric current, then

$$\left. \begin{aligned} 4\pi u &= \frac{d\gamma}{dy} - \frac{d\beta}{dz} \\ 4\pi v &= \frac{d\alpha}{dz} - \frac{d\gamma}{dx} \\ 4\pi w &= \frac{d\beta}{dx} - \frac{d\alpha}{dy} \end{aligned} \right\} \dots\dots\dots (8).$$

The current component u will include the total displacement current in the direction of x , i.e., not only that due to the variation of the induced electromotive intensity, but that also due to the continual alteration in position of the atoms throughout the medium. This will be equal to $\kappa \dot{P}/4\pi$. Mr. Heaviside‡ has further shown that in order to close the displacement current produced by a charge q , moving with a velocity v_1 , we must add at the position occupied by q a current element such that its moment is qv_1 . Hence in the present case the value of u will be given by $\frac{\kappa \dot{P}}{4\pi} + \Sigma qv_x$, where Σqv_x indicates the summation, for unit volume, of the products of the several charges into their respective velocities parallel to the x axis.

Hence (8) may be written

$$\left. \begin{aligned} \kappa \dot{P} + 4\pi \Sigma qv_x &= \frac{d\gamma}{dy} - \frac{d\beta}{dz} \\ \kappa \dot{Q} + 4\pi \Sigma qv_y &= \frac{d\alpha}{dz} - \frac{d\gamma}{dx} \\ \kappa \dot{R} + 4\pi \Sigma qv_z &= \frac{d\beta}{dx} - \frac{d\alpha}{dy} \end{aligned} \right\} \dots\dots\dots (9).$$

* 'Recent Researches in Electricity and Magnetism,' p. 252.

† The equations obtained by Helmholtz, besides differing in sign from the corresponding equations of Maxwell, would lead to the conclusion that the displacement produced by the charged atoms is circual.

‡ O. Heaviside, 'Phil. Mag.,' April, 1882, p. 324; see also J. J. Thomson, 'Phil. Mag.,' April, 1881, July, 1889; also 'Recent Researches,' p. 19.

For a medium whose permeability is unity, we shall finally obtain, eliminating α, β, γ between (9) and (7),

$$\left. \begin{aligned} \nabla^2 P &= \kappa \frac{d^2 P}{dt^2} + 4\pi \frac{d}{dt} \Sigma q v_x \\ \nabla^2 Q &= \kappa \frac{d^2 Q}{dt^2} + 4\pi \frac{d}{dt} \Sigma q v_y \\ \nabla^2 R &= \kappa \frac{d^2 R}{dt^2} + 4\pi \frac{d}{dt} \Sigma q v_z \end{aligned} \right\} \dots\dots\dots (10).$$

8. Consider the propagation, parallel to the axis of z , of a train of plane waves, the electrical disturbances being parallel to the x axis. (10) reduces to

$$\frac{d^2 P}{dz^2} = \kappa \frac{d^2 P}{dt^2} + 4\pi \frac{d}{dt} \Sigma q v_x \dots\dots\dots (11).$$

It only remains to express the last term on the right-hand side in terms of P .

Consider a molecule whose axis, as previously defined, makes an angle θ with the axis of x . For simplicity, suppose both atoms comprised in the molecule to have equal masses. It has been shown by Mr. Heaviside and Professor J. J. Thomson,* that a charged sphere moving with a velocity small in comparison with the velocity of light, has an apparent mass greater than its true mass by $\frac{2}{3} \frac{q^2}{a}$, where q is the charge, and a is the radius of the sphere. When other charged spheres are moving in the neighbourhood, a further correction might be necessary. Let, then, m represent the apparent mass of either atom in the molecule. The differential equation for the rotational displacement of the molecule will be of the form

$$2m \frac{I}{4} \frac{d^2 \phi}{dt^2} + \frac{I}{2} \gamma \frac{d\phi}{dt} + \frac{1}{K_1} \frac{I}{2} \phi - qIP \sin \theta = 0.$$

Here γ is the molecular viscosity, the other letters having the same meaning as in § 4.

If γ is so small that the time of free rotational vibration is not appreciably affected thereby, this equation may be written

$$\frac{d^2 \phi}{dt^2} + \frac{\gamma}{m} \frac{d\phi}{dt} + \frac{4\pi^2}{\tau_1^2} \phi - \frac{2qP}{ml} \sin \theta = 0,$$

where $\tau_1 = 2\pi \sqrt{(K_1 m)} =$ time occupied by a complete rotational vibration.

* O. Heaviside, 'Phil. Mag.,' April, 1889; 'Electrical Papers,' p. 505; J. J. Thomson, 'Recent Researches,' p. 21.

As we are here concerned only with forced vibrations, and therefore the particular integral of the above equation alone is required, we may write the solution to the above equation

$$\phi = \frac{2q \sin \theta}{ml} (D_1^{-1}P) \dots \dots \dots (12),$$

where D_1^{-1} indicates the inverse of the operator D_1 , and

$$D_1 = \frac{d^2}{dt^2} + \frac{\gamma}{m} \frac{d}{dt} + \frac{4\pi^2}{\tau_1^2}.$$

Similarly the equation to the atomic vibration along the axis of the molecule will be of the form

$$m \frac{d^2}{dt^2} \left(\frac{\Delta l}{2} \right) + \gamma \frac{d}{dt} \left(\frac{\Delta l}{2} \right) + \frac{1}{K_2} \frac{\Delta l}{2} - qP \cos \theta = 0.$$

The particular integral of this equation may be written

$$\Delta l = \frac{2q \cos \theta}{m} (D_2^{-1}P) \dots \dots \dots (13),$$

where D_2^{-1} is the inverse of the operator D_2 , and

$$D_2 = \frac{d^2}{dt^2} + \frac{\gamma}{m} \frac{d}{dt} + \frac{4\pi^2}{\tau_2^2},$$

$$\tau_2 = 2\pi \sqrt{(K_2 m)}.$$

It has been assumed that the coefficient of viscosity is the same for both kinds of vibration.

Now the component of $\Sigma p v_x$ contributed by the two atoms composing the molecule under consideration will obviously be obtained by differentiating with regard to time, the expression

$$ql\phi \sin \theta + q\Delta l \cos \theta.$$

Eliminating ϕ and Δl by the aid of (12) and (13), this expression becomes

$$\frac{2q^2}{m} \{ \sin^2 \theta (D_1^{-1}P) + \cos^2 \theta (D_2^{-1}P) \}.$$

Hence employing precisely similar reasoning to that used in § 4, we finally determine that $\Sigma q v_x$ will be obtained* by differentiating with regard to time the expression

* [Added June 13.—The possible presence of free ions, considered merely as isolated charged atoms, is not considered capable of materially affecting the dispersion formula for light waves. This would follow no less from theoretical considerations than from such facts as that the absorption of dilute sulphuric acid is not appreciably different from that of pure water. For very long electrical waves the case would be different.]

$$\begin{aligned} \frac{nq^2}{m} \left\{ (D_1^{-1}P) \int_0^\pi \sin^3 \theta d\theta + (D_2^{-1}P) \int_0^\pi \cos^2 \theta \sin \theta d\theta \right\} \\ = \frac{nq^2}{m} \left\{ \frac{4}{3}(D_1^{-1}P) + \frac{2}{3}(D_2^{-1}P) \right\}, \\ \therefore \frac{d}{dt} \Sigma q v_x = \frac{nq^2}{m} \frac{d^2}{dt^2} \left\{ \frac{4}{3}(D_1^{-1}P) + \frac{2}{3}(D_2^{-1}P) \right\}. \end{aligned}$$

9. Substituting this value in (11), we obtain

$$\frac{d^2 P}{dx^2} - \kappa \frac{d^2 P}{dt^2} = \frac{4\pi nq^2}{m} \frac{d^2}{dt^2} \left\{ \frac{4}{3}(D_1^{-1}P) + \frac{2}{3}(D_2^{-1}P) \right\}.$$

Performing the operation denoted by $D_1 D_2$ on the whole expression, we get

$$\left(\frac{d^2}{dx^2} - \kappa \frac{d^2}{dt^2} \right) (D_1 D_2) P = \frac{4\pi nq^2}{m} \frac{d^2}{dt^2} \left(\frac{4}{3} D_2 + \frac{2}{3} D_1 \right) P \quad \dots (14).$$

Let $P = A_1 e^{\frac{2\pi i}{V_0} (x - V_0 t)} \dots \dots \dots (15).$

Then since $D_1 P = \left(-\frac{4\pi^2}{\tau^2} - i \frac{2\pi\gamma}{m\tau} + \frac{4\pi^2}{\tau_1^2} \right) P$, with a similar expression for $D_2 P$, we obtain as the condition that (15) should be a solution of (14)

$$-\frac{4\pi^2}{V_0^2 \tau^2} + \kappa \frac{4\pi^2}{\tau^2} = -\frac{4\pi nq^2}{m} \cdot \frac{4\pi^2}{\tau^2} \left(\frac{\frac{4}{3}}{\frac{4\pi^2}{\tau_1^2} - i \frac{2\pi\gamma}{m\tau} - \frac{4\pi^2}{\tau^2}} + \frac{\frac{2}{3}}{\frac{4\pi^2}{\tau_2^2} - i \frac{2\pi\gamma}{m\tau} - \frac{4\pi^2}{\tau^2}} \right).$$

Also $\kappa = \frac{1}{V_0^2}$, where V_0 is the velocity of propagation of electrical disturbances in vacuo. Substituting this and simplifying we obtain

$$\frac{V_0^2}{V_2^2} = \mu^2 = 1 + \frac{c_1}{\frac{1}{\tau_1^2} - i \frac{\gamma}{2\pi m\tau} - \frac{1}{\tau^2}} + \frac{c_2}{\frac{1}{\tau_2^2} - i \frac{\gamma}{2\pi m\tau} - \frac{1}{\tau^2}} \quad \dots (16).$$

Here μ is the refractive index of the medium, and

$$c_1 = \frac{4}{3} \frac{nq^2 V_0^2}{m\pi}, \quad c_2 = \frac{2}{3} \frac{nq^2 V_0^2}{m\pi}.$$

For transparent media, γ will be small, and the above equation may be written

$$\mu^2 = 1 + \frac{c_1}{\frac{1}{\tau_1^2} - \frac{1}{\tau^2}} + \frac{c_2}{\frac{1}{\tau_2^2} - \frac{1}{\tau^2}}.$$

Since both c_1 and c_2 are directly proportional to n the number of molecules per unit volume, it follows that in a medium which can be compressed without appreciably altering τ_1 and τ_2 we shall have

$$\mu^2 - 1 \propto \text{density.}$$

When μ is very nearly equal to unity, this may be written

$$\mu - 1 \propto \text{density,}$$

which is Gladstone and Dale's well-known law.

Further, the refractive index for infinitely long waves is obtained from the equation.

$$\begin{aligned}\mu^2_{\infty} &= 1 + c_1\tau_1^2 + c_2\tau_2^2 \\ &= 1 + \frac{4}{3} \frac{nq^2V_0^2}{m\pi} \cdot 4\pi^2K_1m + \frac{4}{3} \frac{nq^2V_0^2}{m\pi} \cdot 4\pi^2K_2m \\ &= 1 + \frac{4\pi M}{K},\end{aligned}$$

which is the value obtained for the specific inductive capacity of a medium by (4).

Further, we may re-write (15),

$$\mu^2 = \mu^2_{\infty} + \frac{c_1\tau_1^4}{\tau^2 - \tau_1^2} + \frac{c_2\tau_2^4}{\tau^2 - \tau_2^2}.$$

$$\text{Also if } V_0\tau_1 = \lambda_1, \quad V_0\tau_2 = \lambda_2, \quad V_0\tau = \lambda,$$

this may be written

$$\mu^2 = \mu^2_{\infty} + \frac{c'\lambda_1^2}{\lambda^2 - \lambda_1^2} + \frac{c''\lambda_2^2}{\lambda^2 - \lambda_2^2} \dots\dots\dots (17),$$

$$\text{where } c' = c_1\tau_1^2, \quad c'' = c_2\tau_2^2.$$

This is Ketteler's dispersion formula, which he has shown is capable of representing the optical properties of a large number of substances over a great range of values of λ . It is the same as that obtained by Mr. Glazebrook in his paper "On the Extension of Lord Kelvin's Contractile Æther to include Dispersion, &c.,"* and by a slight alteration in form reduces to Lord Kelvin's dispersion formula.† As its properties have been fully discussed in the papers referred to, nothing further need be said about it here.

The molecular constitution considered above is, of course, the simplest imaginable. With a more complicated molecule, since each

* R. T. Glazebrook, 'Phil. Mag.', December, 1888.

† Lord Kelvin, Baltimore Addresses, 1884.

atom may have its own vibration periods, a much more complicated dispersion formula might be anticipated. In this connection the fact that the absorption spectra of the permanganates of potassium, sodium, lithium &c., are identical* appears to be very suggestive.

For infinitely quick vibrations μ becomes equal to unity. If the radiations discovered by Röntgen are ultimately proved to be due to transverse periodic disturbances of the ether, of very short wave length, this would explain why no refraction of these radiations is produced by material media.

10. The application of (16) to explain the optical properties of crystals and metals is obvious. In a crystal the co-efficients of the terms involving τ_1 and τ_2 will depend on the direction of vibration, which will result in μ varying with the direction of propagation of light. Further, in accordance with a well-known mechanical principle γ must be taken into account, when τ is nearly equal to τ_1 or τ_2 . In this case (16) may be reduced to the form

$$\mu^2 = R^2 (\cos 2\alpha + i \sin 2\alpha).$$

$\sin 2\alpha$ is essentially positive, whilst $\cos 2\alpha$ may be either positive or negative. This is generally considered sufficient to explain the optical properties of metals, and of the quasi-metallic aniline dyes when in a solid state.

Kundt has pointed out that the velocity of red light in a metal is proportional to the electrical conductivity of that metal. A suggestive relation in this connection may be derived from (16).

Let $2\alpha = \pi - 2\alpha'$. Experiments with metallic films and prisms alike show that $2\alpha'$ is small for the majority of metals. We may write

$$\mu = iR (\cos \alpha' - i \sin \alpha').$$

The real part of the refractive index is therefore equal to $R \sin \alpha'$.

(16) may also be written in the form

$$\mu^2 = f(\tau) + i\gamma F(\tau).$$

Then, since $\sin 2\alpha' = \tan 2\alpha' = \frac{\gamma F(\tau)}{-f(\tau)}$,

$$R \sin \alpha' = \frac{\gamma F(\tau)}{2\sqrt{-f(\tau)}}, \text{ approximately.}$$

Mr. Tomlinson† has shown that in a number of cases the molecular viscosities of metals are in the same order of magnitude as their

* Ostwald, 'Zeits. Phys. Chem.,' vol. 9, p. 579.

† H. Tomlinson, 'Phil. Trans.,' 1883, p. 168.

electrical resistances. Hence if γ in the present instance might be identified with the molecular viscosity as determined by Mr. Tomlinson, a connection similar to that derived experimentally by Kundt would be established. Calculating by the above approximate process the real part of the refractive index from Drude's experimental data, a fair agreement, as to order of magnitude, is found with the same quantity when calculated accurately. The agreement is not, however, close enough to explain the very accurate proportionality between the velocity of light in a metal and the conductivity of the latter which Kundt's figures imply. Since, however, Pflüger has shown that the temperature co-efficients of the velocities of light and the conductivities are not of the same order of magnitude, the process employed above may perhaps represent the nature of the physical connection between the two quantities to a sufficient degree of approximation.

11. [*Added May 10.*—Another relation of some importance can be readily obtained from (17). It is a well-established experimental law, often]made the basis of exact chemical determinations, that the co-efficient of absorption of a solution of an absorbent substance in a transparent liquid is proportional to the number of molecules of the absorbent present in unit volume of the solution. A simple extension of the reasoning formerly used will give for the square of the refraction index of such a mixture the value

$$\mu^2 = 1 + \frac{c_1 n_1}{\frac{1}{\tau_1^2} - \frac{1}{\tau^2}} + \Sigma \frac{c_2 n_2}{\frac{1}{\tau_2^2} - \frac{\gamma}{2\pi m \tau} - \frac{1}{\tau^2}} \dots \dots \dots (18),$$

where c_1, n_1, τ_1 are constants for the transparent medium, whilst c_2, n_2, τ_2, m , and γ refer to properties previously defined of the dissolved absorbent substance. Further we may write

$$\mu^2 = A + m_2 B,$$

where A , in dilute solutions, is nearly independent of the amount of colouring matter present, whilst

$$B = \frac{c_2 \frac{\gamma}{2\pi m \tau}}{\left(\frac{1}{\tau_2^2} - \frac{1}{\tau^2}\right)^2 + \frac{\gamma^2}{4\pi^2 m^2 \tau^2}} \dots \dots \dots (19),$$

for a substance possessing only one absorption band, and is to a first approximation independent of the properties of the solvent.

Then $\mu^2 = R^2(\cos 2x + i \sin 2x) = A + m_2 B.$

When α is small, $\sin 2\alpha = \tan 2\alpha = \frac{n_2 B}{A}$,

$$R^2 = \sqrt{(A^2 + n_2^2 B^2)} = A, \text{ approximately.}$$

Hence
$$\mu = \sqrt{A + \frac{n_2 B}{\sqrt{A}}}.$$

Here $n_2 B / \sqrt{A}$ is the coefficient of absorption of the solution, and is proportional to n_2 , the number of molecules of the absorbent substance present per unit volume. Since it is essential that (19) should possess a well-marked maximum value for a certain wave-length (that of the light absorbed), additional evidence is obtained in support of the introduction of a viscous term into the equations to the atomic vibrations.

The equation for the square of the refractive index of a substance like crystallized copper sulphate, which possesses marked selective absorption without exhibiting selective reflection to any extent, might be represented by an equation of the form of (18); τ_2 , &c., now referring to the conditions of motion of a particular atom, the motions of the remaining atoms giving rise to the terms involving τ_1 , &c.

“A Photographic Investigation of the Absorption Spectra of Chlorophyll and its Derivatives in the Violet and Ultra-violet Region of the Spectrum.” By C. A. SCHUNCK. Communicated by Dr. E. SCHUNCK, F.R.S. Received March 17,—Read March 24, 1898.

[PLATES 3, 4, 5.]

As is well known from the investigations of Soret,* Gamgee,† and others, hæmoglobin and its coloured derivatives show a characteristic absorption band lying between the lines G and M of the solar spectrum. The band has been shown to vary in position between narrow limits; in some derivatives it is nearer the red, and in others nearer the violet, end of the spectrum, and is of all the blood absorption bands the most stable.

The very near relationship that has been shown to exist by Schunck and Marchlewski‡ between phylloporphyrin (a chlorophyll derivative) and hæmatoporphyrin (a hæmoglobin derivative), and the remarkable resemblance of their absorption spectra—one may almost

* ‘Arch. des Sciences Phys. et Nat.,’ vol. 61, p. 322; vol. 66, p. 429.

† ‘Arch. des Sciences Phys. et Nat.,’ Dec., 1895.

‡ ‘Roy. Soc. Proc.,’ vol. 59, p. 233.

say they are identical—led Tschirch to the belief that there must also exist a like resemblance in the violet and ultra-violet region of the spectrum. Tschirch in his investigations* shows this to be the case, and finds a band in phylloporphyrin (termed by him phyllo-purpuric acid) in the same position as the band observed in hæmatoporphyrin, and this corresponding to the characteristic band which has been shown to exist in the blood colouring matter derivatives. But further on I will show that this particular resemblance in these two derivatives is only partly correct, the band in the phylloporphyrin I examined being double, though occupying the same position as the single one of hæmatoporphyrin.

Tschirch (in the same memoir) makes the further important discovery that this band is not confined to phylloporphyrin alone, but that it is distinctive of the chlorophyll derivatives generally, and that it occupies (in the derivatives he observed) the same position (varying between narrow limits) as that shown to exist in the blood derivatives, and, like the blood band, the new chlorophyll band shows much greater stability than any of the other bands, no matter what chemical changes the derivatives have undergone.

The chlorophyll derivatives investigated by Tschirch are phyllo-purpuric acid (impure phylloporphyrin), phyllocyanic acid (phyllocyanin), and its zinc, copper, and iron compounds, in all of which he finds a single band corresponding in position with the band in the hæmoglobin derivatives, in the zinc and copper compounds shifted slightly towards the red end of the spectrum, and he also finds it indicated in the living leaf, whilst in chrysophyll and carotin he observes three bands situated between the solar lines F and H and in identical positions. By the kind permission of Dr. E. Schunck I have had access to his beautiful collection of chlorophyll derivatives, and have been able to examine spectroscopically in the *pure* state solutions of chrysophyll, carotin, chlorophyll, phylloxanthin, phyllocyanin and its zinc and copper compounds, alkachlorophyll, phyllotaonin, ethyl-phyllotaonin, and phylloporphyrin, and a specimen of hæmatoporphyrin kindly sent me by Dr. L. Marchlewski. In all there appears a characteristic absorption between the solar lines F and M, which confirms the statement of Tschirch that the chlorophyll derivatives, like the hæmoglobin derivatives, give a very characteristic absorption in this region of the spectrum, and considering the very near relationship which exists between phylloporphyrin and hæmatoporphyrin, is further evidence of the supposition that there exists something in common to both chlorophyll and hæmoglobin, the two great colouring matters of biological importance in the vegetable and animal kingdoms.

* '*Berichte der Deutschen Botanischen Gesellschaft*,' vol. 14, Part 2, p. 76, 1896.

As I have been able to examine many more derivatives than the former investigator, and as the results of my observations differ from his, inasmuch that I find only some of the derivatives are characterised by the *single* band, whilst in others two are apparent, and in chlorophyll itself and chrysophyll three, and also that my method of procedure differs from his, it will perhaps be of interest to give the results of my experiments as compared with his.

Method of Procedure.

The spectroscope was a single prism one of Iceland spar, which just divided the sodium lines. The lenses were of quartz, the focal length of the collimator lens being 12·5 inches, and that of the camera lens 42 inches. The dark slide which held the plate was movable at fixed intervals, so that seven exposures could be taken on the one plate. The solutions were examined in a glass cell with parallel quartz faces, placed in front of the slit, the whole apparatus being set up for me by Mr. A. Hilger, of London. The source of light used was a Welsbach incandescent mantle of sixty-candle power, no chimney being used, placed 8 inches distant from the slit. For reference lines a hydrogen vacuum tube was used, from which the lines F, S', L, and H' were obtained, and the violet potassium line K_{β} was at the same time thrown in by volatilising a little of the salt in a Bunsen burner in front of the slit. The plate used was Messrs. Cadett and Neal's "lightning," pyrosoda being adopted as the developer. The exposure given in each case was half an hour, which was found to be the most advantageous after various trials of different times of exposure had been made. Under these conditions the photographs extended distinctly as far as the solar line Q. On each plate a spectrum of the source of light used was thrown in for a comparison with the light absorbed by the solutions of the different derivatives. In every case the solutions had to be excessively diluted before the characteristic absorption became apparent on the photographic plate, so dilute that, with the exception of chrysophyll, carotin, and chlorophyll, only a faint indication of colour was visible in the solutions to the eye by transmitted light, and of the bands in the visible region of the spectrum, only the first, the characteristic one in the red, was visible, and that in the majority of cases only faint. In the case of the hydrochloric acid compounds of phylloporphyrin and hæmatoporphyrin one might say the solutions were colourless, and yet these two derivatives give the *single* band more pronounced and better defined than any of the others. As a very slight difference in the strength of the solutions gave an appreciable difference in the resulting absorption spectra, three solutions of *slightly* varying strengths were photographed of each derivative, and the most charac-

teristic of each were then selected and photographed together as depicted in the plates. In no case on dilution were any further bands observable, and in all I find this particular distinctive absorption situated between the lines F and L in chrysophyll, carotin, and chlorophyll, and in the other derivatives between the lines G and L, the mean position of the bands being situated at the K_{β} line, varying sometimes slightly towards red and in others towards the violet. The main difference in my procedure to that of Tschirch is that I have made use of artificial light in the place of direct sunlight. By this means I have been able to reproduce the bands as regards their definition and their relative intensity in a far more distinct manner, and though my photos. do not extend so far as his, mine going to Q and his to S, yet as the characteristic absorption does not extend further than M this is not of great consequence.

Chlorophyll, Chrysophyll and Carotin.

The chlorophyll solution was prepared in the usual way. Fresh leaves were extracted with boiling alcohol, and the solution filtered off from the fatty deposit which usually forms on standing. When the solution is diluted so that in the normal chlorophyll spectrum of four bands only the characteristic one in the red is visible when received by the spectroscope, and a photograph is now taken, three bands are found lying between the lines F and K_{β} (Plate 3, figs. 1 and 2). The first two bands are the bands usually numbered 5 and 6 of the normal chlorophyll spectrum, and can be seen on dilution by the eye when sunlight is used, and the first one, 5, when artificial light is used. But the third band in the violet, having its centre situated about the line h , has not, I believe, been observed before. In weaker solutions still, one only gets a general absorption in the ultra-violet, no further bands being discernible. As is well known, chlorophyll solutions prepared from the leaves of different plants vary slightly in their absorption spectra, which depends upon the amount of acid present in the leaf. In the spectrum of the purest chlorophyll solutions, the fourth band situated about the line E is extremely faint; the purer the solution the fainter it is, but where the least trace of an acid is present this band appears darker than the third, situated between the lines D and E, and on the solutions standing becomes darker still, while the third becomes fainter, and has moved further away from the red end. If, however, a chlorophyll solution be exposed to the action of direct sunlight for a few hours until the colour has become brown, or if a few drops of a *strong* acid, as hydrochloric, be added, and the solution be allowed to stand for a few days, a further change in the spectrum takes place, by the formation of a fifth band situated between the lines

E and F. According to the investigations of Schunck and Marchlewski,* these changes may be explained by the supposition that phylloxanthin is formed in the first case, and phyllocyanin in the second. These changes take place in a greater or less degree in every chlorophyll solution on standing, according to the amount and strength of acid present originally.

I have found in all the various chlorophyll solutions I have examined, even in those which from the commencement gave the fourth band darker than the third, and those in which the same change had taken place on standing, that no change had taken place in the violet, the only change being that more and more of the ultra-violet rays are absorbed. In the purest solution the total obscuration started at the line N, while in others the total obscuration commenced at the line H₁. But in the case of a solution which had been exposed to the action of direct sunlight and which showed the fifth band, then the bands in the violet had disappeared altogether, leaving only a general absorption in the ultra-violet. As will be seen from Plate 5 (figs. 2 and 3), the second and third of these violet bands are identical in position with the two bands found in phylloxanthin, while the first corresponds in position to a new sixth band I have found to exist in the spectrum of phyllocyanin when the solution is examined in a more concentrated form than usual, and which appears distinct but faint when photographed.

The chrysophyll was obtained by Schunck's method† and examined in an alcoholic solution; the carotin crystals were obtained from the carrot root by the process of Arnaud, and was likewise examined in an alcoholic solution. In each three very distinct characteristic bands were found, which agrees with the statement of Tschirch, but the bands from my photographs are not in identical positions, those in the carotin being slightly shifted towards the violet end.

Like Tschirch, I find that both these derivatives are very transparent to the ultra-violet rays. From Plate 3 (figs. 2 and 3) it will be seen that the three chrysophyll bands occupy intermediate positions compared to the three chlorophyll bands, which seems to point to the supposition that chrysophyll does not exist in chlorophyll solutions as such, but under certain conditions only is formed by decomposition of a derivative,‡ and then crystallises out, or that if it does exist in the solutions it must be in a very small quantity, otherwise the bands would overlap each other, and the result would be a total obscuration, which I have only found to be the case in a chlorophyll solution exposed to the action of direct sunlight.

* 'Roy. Soc. Proc.,' vol. 57, p. 321.

† 'Roy. Soc. Proc.,' vol. 44, p. 449.

‡ Hansen, 'Die Farbstoffe des Chlorophylls,' 1889, p. 58.

Phylloxanthin, Phyllocyanin and its Compounds.

Phylloxanthin and phyllocyanin, the two leading chlorophyll derivatives obtained by the action of hydrochloric acid (Schunck, "Contributions to the Chemistry of Chlorophyll")* give in alcoholic solutions in the violet region of the spectrum, in the former case two bands, and in the latter one (Plate 5, figs. 3 and 4). As has already been pointed out, the two phylloxanthin bands are identical in position with the second and third of the chlorophyll ones in the violet, the band in phyllocyanin being moved slightly towards the violet end, this band being situated between the lines h and H^2 . In both cases the solutions have to be exceedingly dilute before the bands become visible on the photographic plate, the only band remaining visible to the eye in the visible part of the spectrum being the first, the characteristic one in the red, and that now only appears faint. From the investigations of Schunck,† phyllocyanin plays the part of a weak base, and combines with strong acids, the compounds, however, being unstable and easily decomposed even by water, and, like other bases, giving definite double compounds of great comparative stability, into which metals and acids, especially organic acids, enter as constituents.

I have examined the compounds formed by dissolving phyllocyanin in *anhydrous* acetic acid, hydrochloric acid, and sulphuric acid, and find a more pronounced band in each than even in the case of an alcoholic solution of phyllocyanin, but in each compound the band is shifted slightly towards the red end of the spectrum compared to the band in phyllocyanin itself (Plate 4, figs. 1, 2, 3, and 4).

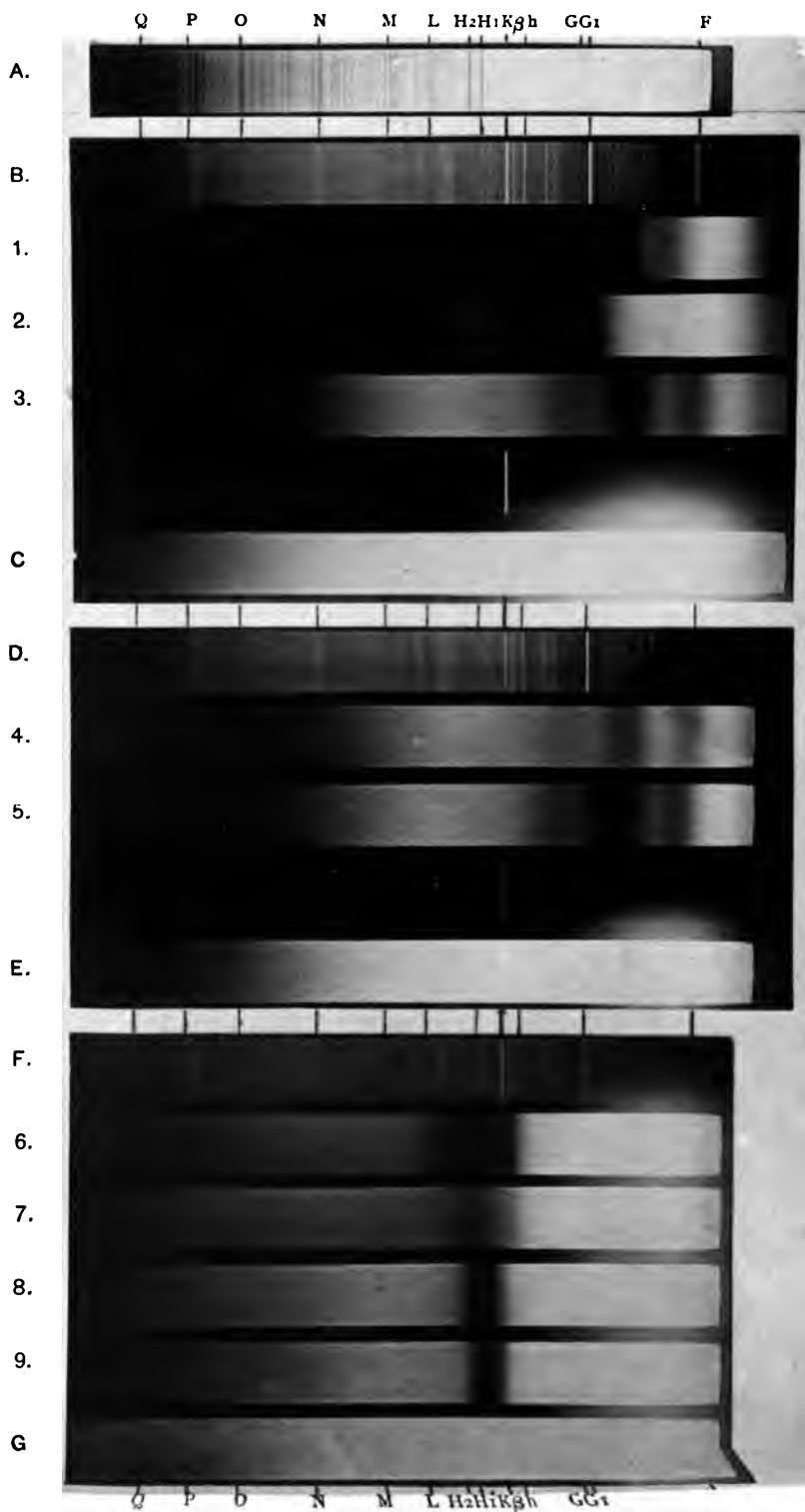
Of the compounds formed by phyllocyanin with metallic salts, the one with zinc carbonate in alcohol, with zinc acetate in acetic acid, and the one with cupric acetate in the same solvent were examined. In each the characteristic absorption was noticeable in the same region of the spectrum. In the zinc carbonate compound two very distinct bands were found corresponding very nearly in position with the two in phylloxanthin, the shifting being towards the violet end (Plate 4, figs. 5 and 6), the zinc acetate compound, on the other hand, gave only one band, corresponding in position with the band of the anhydrous acetic acid compound of phyllocyanin (Plate 4, fig. 7), whilst in the case of the cupric acetate compound two badly defined dark bands were apparent, the division between the bands corresponding about to the position of the phyllocyanin band (Plate 4, fig. 8). In both these series of compounds, as in the case of phylloxanthin and phyllocyanin, the solutions had to be very dilute before the characteristic absorption became apparent on the photographic

* 'Roy. Soc. Proc.,' vol. 39, p. 348; vol. 50, p. 306.

† 'Roy. Soc. Proc.,' vol. 39, pp. 354 and 356.

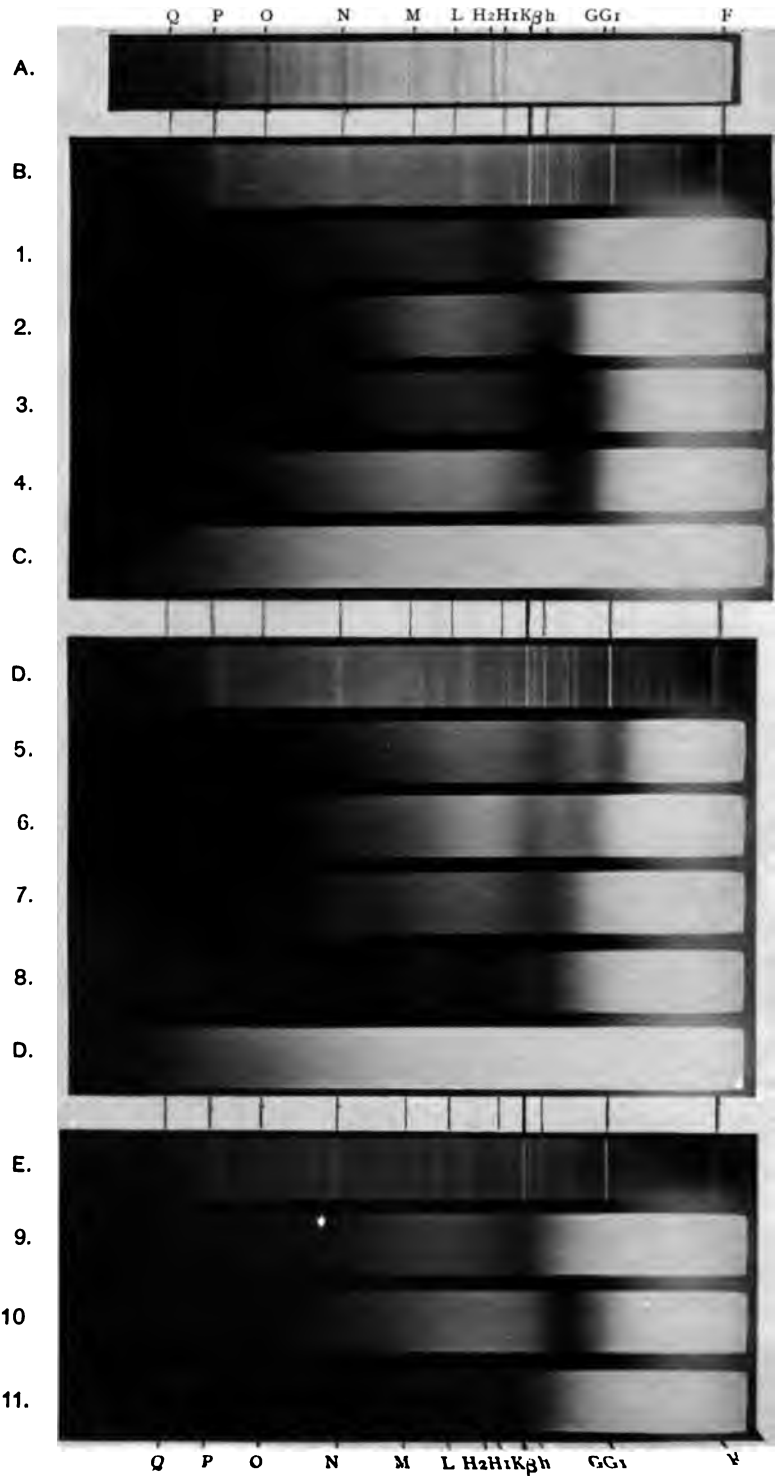
Explanation of Plate 3.

- A. Solar Spectrum.
- B. Reference Lines.
- 1. Chlorophyll in Alcohol.
- 2. Chlorophyll Diluted in Alcohol.
- 3. Chrysophyll in Alcohol.
- C. Spectrum of the Incandescent Mantle.
- D. Reference Lines.
- 4. Chrysophyll in Alcohol.
- 5. Carotin in Alcohol.
- E. Spectrum of the Incandescent Mantle.
- F. Reference Lines.
- 6. Phylloporphyrin in Alcohol.
- 7. Hæmatoporphyrin in Alcohol.
- 8. HCl Salt of Phylloporphyrin in Water.
- 9. HCl Salt of Hæmatoporphyrin in Water.
- G. Spectrum of the Incandescent Mantle.



Explanation of Plate 4.

- A. Solar Spectrum.
- B. Reference Lines.
- 1. Phyllocyanin in Alcohol.
- 2. Phyllocyanin in Anhydrous Acetic Acid.
- 3. Phyllocyanin in HCl.
- 4. Phyllocyanin in H_2SO_4 .
- C. Spectrum of the Incandescent Mantle.
- D. Reference Lines.
- 5. Phylloxanthin in Alcohol.
- 6. Phyllocyanin Zinc Carbonate in Alcohol.
- 7. Phyllocyanin Zinc Acetate in Acetic Acid.
- 8. Phyllocyanin Cupric Acetate in Acetic Acid.
- D. Spectrum of the Incandescent Mantle.
- E. Reference Lines.
- 9. Alkachlorophyll in Alcohol.
- 10. Sodium Salt of Alkachlorophyll in Alcohol.
- 11. Sodium Salt of Alkachlorophyll in Water.







Explanation of Plate 5

A. Solar Spectrum.

B. Reference Lines.

1. Chrysophyll in Alcohol.
2. Chlorophyll in Alcohol.
3. Phylloxanthin in Alcohol.
4. Phyllocyanin in Alcohol.
5. Phyllocyanin Zinc Carbonate in Alcohol.

C. Spectrum of the Incandescent Mantle.

D. Reference Lines.

6. Alkachlorophyll in Alcohol.
7. Phyllotaonin in Alcohol.
8. Ethyl-Phyllotaonin in Alcohol.
9. Phylloporphyrin in Alcohol.
10. HCl Salt of Phylloporphyrin in Water.

E. Spectrum of the Incandescent Mantle.

Schunck.

Roy. Soc. Proc., Vol. 63, Plate 5.

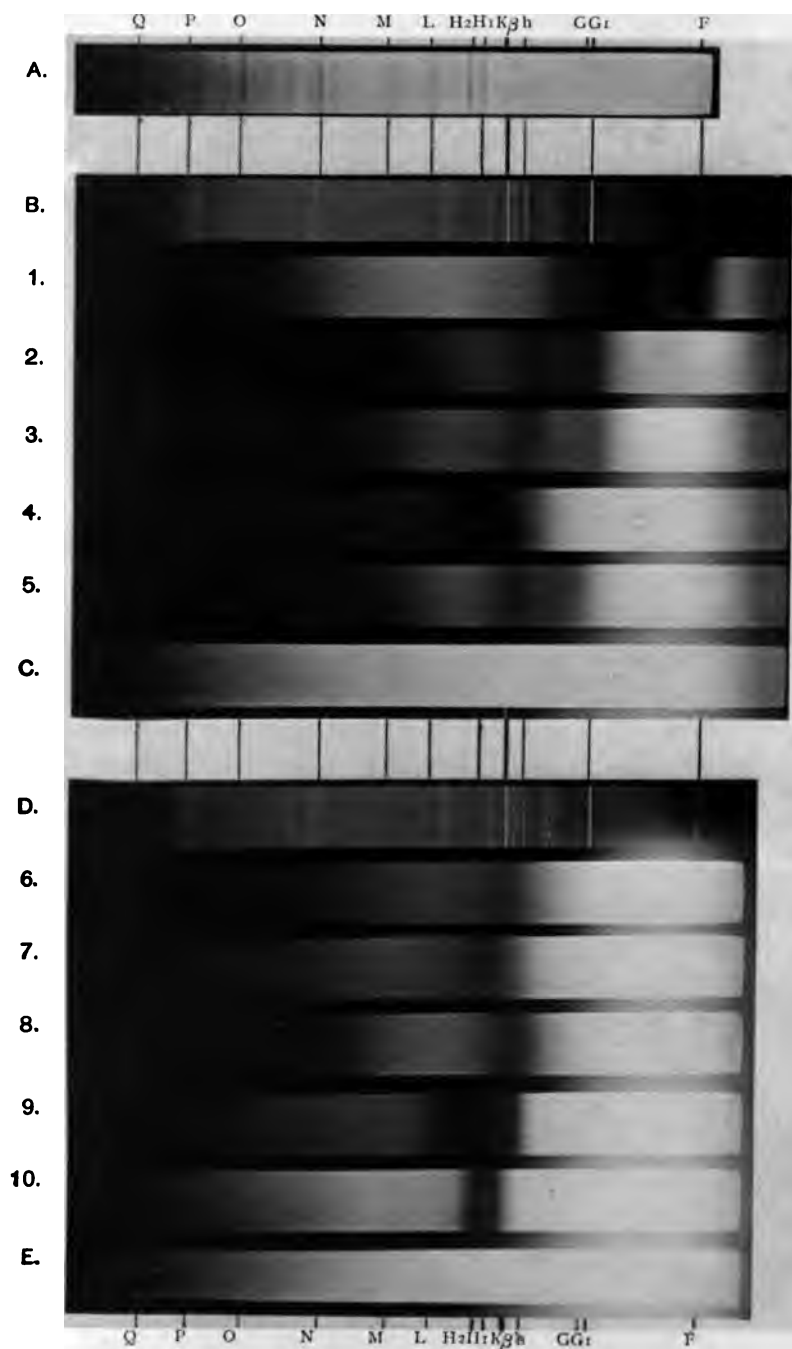


plate. Here the results of my experiments differ from those of Tschirch, who found but one band in the zinc and copper compounds, but the discrepancy may have arisen from the fact that he does not state in his memoir what acid was in combination along with the metal with phyllocyanin. That the zinc carbonate compound of phyllocyanin should give two bands in the violet region corresponding very nearly in position and relative intensity with the two of phylloxanthin, and therefore, with the second and third violet chlorophyll bands, is interesting from the fact that of all the chlorophyll derivatives the spectrum of the phyllocyanin zinc carbonate in the visible region corresponds more closely than any of the others with that of chlorophyll, and from the deductions arrived at from this similarity by Schunck* as regards the functions of chlorophyll as being a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy.

Alkachlorophyll, Phyllotaonin, Ethyl-phyllotaonin, Phylloporphyrin.

In his "Contributions to the Chemistry of Chlorophyll" Schunck has discovered that by the action of alkalis upon chlorophyll, the above definite derivatives are produced in a crystalline form.† Alkachlorophyll, phyllotaonin, and ethyl-phyllotaonin all give the *single* band in a pronounced and well-defined manner, corresponding very nearly in position with the band in phyllocyanin, viz., at about K_{β} , in phyllotaonin it being shifted slightly towards the violet, and in the other two a trifle towards the red end of the spectrum (Plate 5, figs. 6, 7, and 8). Alkachlorophyll also forms a definite crystallised sodium salt. In an alcoholic solution the band appears considerably shifted towards the red, but a watery solution gives the band much obscured, in the same position as the one in the alcoholic solution of alkachlorophyll itself (Plate 4, figs. 9, 10, and 11).

Phylloporphyrin on the other hand gives a double band, intense and fairly well defined, the less refrangible one having its centre situated at the K_{β} line (Plate 5, fig. 9). Phylloporphyrin forms a compound with acids, which has a spectrum in the visible region quite distinct from the very characteristic one of phylloporphyrin itself, which consists of seven bands, while the acid compounds only show three bands.‡ On examining the hydrochloric acid compound in the violet region, a corresponding change is also found in the spectrum, and instead of the double band, a single very intense

* 'Annals of Botany,' vol. 3, pp. 65—120.

† 'Roy. Soc. Proc.,' vol. 39, p. 355; vol. 44, pp. 449—454; vol. 50, pp. 312—316; vol. 57, pp. 316—319.

‡ Schunck and Marchlewski, 'Roy. Soc. Proc.,' vol. 57, p. 319.

one takes its place, situated between the other two (Plate 5, fig. 10). This band is the most pronounced and the best defined one in the whole series, and only becomes visible on the photographic plate in excessively dilute solutions, so dilute that one might say the solution was colourless to the eye when viewed by transmitted light.

Phylloporphyrin and Hæmatoporphyrin.

On comparing the spectra of phylloporphyrin and hæmatoporphyrin in this region, and also those of their hydrochloric acid compounds (Plate 3, figs. 6, 7, 8, and 9), it was found that hæmatoporphyrin gave only a single band, but situated in the same position as the double one of phylloporphyrin. On this point the results of my experiments differ from those of Tschirch, who, as stated above, found in both a *single* band occupying the same position. In the hydrochloric compounds of hæmatoporphyrin, however, a single band of the same pronounced character as that in phylloporphyrin was found, the one in hæmatoporphyrin, as will be seen from the figures, being slightly shifted towards the red end of the spectrum, which is interesting from the fact that just in the same way are the bands in the visible region of the spectrum of these two compounds shifted, this constituting their only spectroscopic difference.

In conclusion, my thanks are due to Dr. E. Schunck and Dr. L. Marchlewski for the valuable assistance they have given me in many details in connection with this investigation.

I hope in a further paper to investigate more particularly the spectroscopic behaviour in the same region of the spectrum of the yellow colouring matter accompanying chlorophyll in leaves and allied colouring matters obtainable from other sources than the leaf, for instance, carotin.

“On Photographic Evidence of the Objective Reality of Combination Tones.” By R. W. FORSYTH, A.R.C.S., and R. J. SOWTER, A.R.C.S. Communicated by Professor RÜCKER, Sec.R.S. Received March 29,—Read May 5, 1898.

[PLATES 6, 7.]

In the following paper we propose to describe a series of photographs which prove the objective reality of difference and summation tones. The work was suggested to us by Professor Rücker, and we have used the method of detecting these tones which has been described by Rücker and Edser in the ‘Philosophical Magazine’ for April, 1895.

The resonator they employed was a tuning fork. On to one prong of the fork was fixed a mirror, which was made one of a system by which Michelson's interference bands were produced. To the other prong was attached a wooden square of larger area, but of the same weight as the mirror. The fork was then compared with a König standard fork and its frequency adjusted to 64. The notes were produced by a Helmholtz wind siren placed between a wooden pyramidal tube and a large König resonator tuned to 64. The narrow end of the pyramid was placed about half an inch from the wooden square attached to the resonating fork.

Throughout the experiments we used blue light, obtained by passing a beam from an electric lamp through a cell containing an ammoniacal solution of copper sulphate. A slit about 2 inches long and one-twentieth of an inch broad was cut out of a piece of tin-foil pasted on glass, and was placed horizontally across the middle of the bands, so that the bright and dark bands appeared as bright and dark spots respectively.

In our earlier experiments, we took photographs upon flexible film fastened by india-rubber bands to a rotating drum. A ball shutter, such as is used in instantaneous photography, was employed. Working at night, after the traffic had subsided, the bands in their normal condition were perfectly steady. On taking a photograph, each bright band produced a perfectly straight line upon the rotating film, and the whole picture was made up of a series of parallel straight lines.

On sounding a 64-fork in the vicinity of the apparatus, the mirror is set in motion by resonance and thus the bands execute harmonic motions, with a frequency of 64, about their mean positions. The general appearance as then seen by the eye is a blue blur. When this blur was photographed by means of the slit and rotating drum, we obtained a series of sinuous lines. To prove that the frequency of these curves corresponded to a note of 64 vibrations per second, we made observations on the rate of turning of the drum. The following are some of the actual figures obtained:—

Circum. of drum = 31.5 cm. 100 revs. in 19.5 secs.

Wave-length = 2.53 cm.

$$\text{No. of vibns. per sec.} = \frac{100 \times 31.5}{19.5 \times 2.53} = 63.9.$$

We thus obtained a series of photographs of what may be called the difference and summation curves, which were exhibited in a preliminary communication read by Professor Rücker at the recent meeting of the British Association held in Toronto.

The details of the method now employed are different, and we

venture to think much better suited for obtaining good photographs, though the drum enabled us to determine the frequency of the oscillation more easily. When the drum was used, it was inconvenient to take more than one photograph on the same film, and moreover, it was impossible to use plates, which in some respects have advantages over films.

Having satisfied ourselves by means of the drum, that the frequency of the curves we obtained corresponded to 64, we had recourse to a sliding plate arrangement fitted with an automatic shutter. The sliding piece is capable of carrying 3 feet of plate 2 inches in width, and the shutter is so arranged that any 6 inches of the plate can be exposed at will. With one filling of the slide, it is therefore possible to take six photographs.

Our first effort was to obtain photographs of the steady bands when no sounds were produced. We have taken many of these, and reproduce one of them in (Plate 6) fig. 1.

Fig. 2 shows a photograph taken when a 64 fork was sounding rather loudly.

Fig. 3 shows an extraneous disturbance produced by the slamming of a door. It is evident that the vibrations are compound and, in part at all events, forced. We then proceeded to obtain the difference-tone. The frequencies of the two notes used were 256 and 320, and these were produced as by Rücker and Edser with a Helmholtz double wind siren. The 12 row of holes was tuned to a 256-fork, and then to produce the 320-note the 15 row of holes was opened.

Fig. 4 shows a photograph taken when the 256-fork was sounding, and when the siren was sounding in unison with it, the 12 row of holes being open. It will be seen that the bands are steady. There is a slight vibration present, probably due to the disturbance produced by the blowing of the bellows.

Fig. 5 shows a photograph taken immediately after the foregoing one, upon the same plate, and under exactly the same conditions with the single exception, that the 15 row of holes was opened and the 320-note therefore sounding in conjunction with the 256. The objective reality of the difference-tone is clearly proved. This experiment has been repeated on several occasions and with different notes, and we have obtained many photographs demonstrating the vibratory motion which is given to the bands by the difference-tone. In all cases, we have proved that the separate notes sounding alone produced no effect upon the bands.

Fig. 6 (Plate 7) is another photograph of the effects produced by a difference-tone.

It will be observed in these two photographs, that although the chief effect is that due to the sounding of a 64-note, there is super-

Fig.
1.



Fig.
2.



Fig.
3.



Fig.
4.

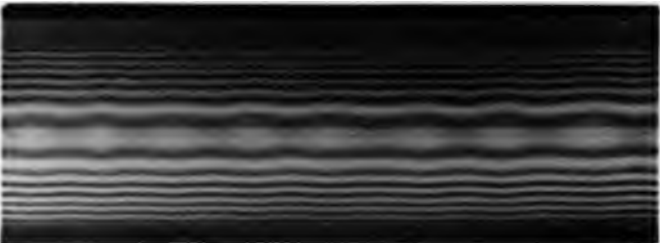


Fig.
5.

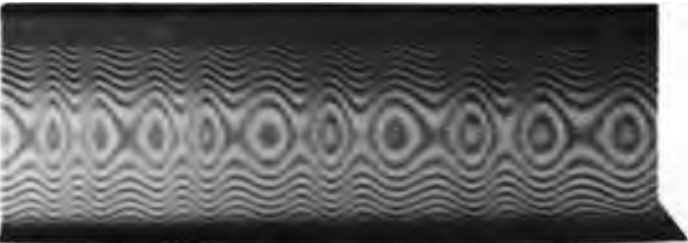


Fig.
6.



Fig.
7.



Fig.
8.



Fig.
9.



imposed upon this some other disturbance which causes a slight departure from the perfect regularity of the curves. This disturbing effect is probably due to the fact, that, as it is difficult to keep the pitch of the notes given by the siren absolutely constant, they had departed somewhat from their proper values at the moment when the photograph was taken, and thus forced vibrations of a pitch slightly different from that of the tuning fork were added to those corresponding to its natural period.

Having finished experiments on the difference-tone, we proceeded to photograph effects produced by the summation tone. The two notes used were obtained from the 9 and 12 rows of holes of the upper box of the Helmholtz double wind siren. It is easily seen that, to give a summation tone of 64, the disc must be revolving $64/(9+12) = 3.048$ times per second. To obtain this rate of rotation we used a stroboscopic method. On the upper surface of the lower box of the siren, we affixed a star-like disc with 18 rays, and viewed it through slits carried by a fork having a frequency of 27.4.

When the star appears stationary, the disc is revolving at the desired rate, for $18 \times 3.048 = 27.4 \times 2$ approximately.

We have taken photographs of the steady bands when the siren has been going at the proper speed, and one set of holes open only. These are exactly like the steady bands obtained in the former cases. On sounding the two notes together the summation tone is produced, and we have photographed it in the manner already described. Figs. 7, 8, and 9 show some of the photographs obtained.

In fig. 9, where the amplitude of the vibration of the bands is large, the plates used were not sufficiently sensitive to photograph them when moving through their mean positions. When the bands are in their extreme position and therefore at rest, the exposure is inversely proportional to the velocity of the plate. But when the bands are passing through their mean position, the exposure is inversely proportional to the velocity found by compounding the velocity of the plate with the velocity of the bands in a direction at right angles. If the amplitude is large enough, this velocity may be so great as to render the time of exposure too small to affect the plate. This phenomenon is slightly noticeable in fig. 5, and is very well marked in figs. 8 and 9.

In conclusion, we wish to thank Mr. Cameron for assisting us in taking some of the later photographs, and Mr. Chapman for the help he has given us in preparing the lantern slides and prints.

“ On the Cytological Features of Fertilisation and related Phenomena in *Pinus silvestris*, L.” By VERNON H. BLACKMAN, B.A., F.L.S., Hutchinson Student, St. John’s College, Cambridge, and Assistant, Department of Botany, British Museum. Communicated by FRANCIS DARWIN, F.R.S. Received May 3,—Read May 26, 1898.

(Abstract.)

This paper gives a fairly complete account of the minute cytological details of the act of fertilisation and of the processes surrounding it, from the formation of the ventral canal cell up to the period of cell-wall formation at the base of the egg.

As the oosphere nucleus, after separation of the nucleus of the ventral canal cell, moves rapidly back towards the centre of the egg, it increases greatly in size, as described by Strasburger. This increase in size is shown to be due to the appearance in the nucleus of a peculiar metaplasmic substance, which fills up the nucleus, and, owing to its attraction for stains, ultimately obscures the chromatin. The mature female nucleus, which is sometimes large enough to be visible to the naked eye, exhibits merely an uniformly staining reticulum composed chiefly of metaplasmic substance, with one or more nucleoli.

By the rupture of the closing membrane of the well-marked pit at the apex of the pollen tube, almost the whole of the contents of the lower part of the tube pass over into the oosphere. At this stage all the four nuclei, together with a considerable number of starch grains from the pollen tube, are to be seen lying in the cytoplasm of the egg. Cytoplasm from the pollen tube must also necessarily pass over, and with it the plastid-like structures to be seen earlier in the cytoplasm of the generative cells.

The behaviour of the four nuclei in the egg was carefully followed; the stalk cell nucleus, the pollen tube nucleus, and one generative nucleus remain at the apex of the egg, near the point of entry, and ultimately become disorganised. The other generative nucleus, which possesses distinct nucleoli, as does also its sister nucleus, advances very rapidly towards the female nucleus, increasing somewhat in size and in mass of staining material on its way. After coming in contact with the much larger female nucleus it gradually penetrates the substance of the latter until it is almost completely enclosed within it, but breaking down of the nuclear walls, that is, actual fusion, is for some time delayed. After fusion has taken place, but while the outlines of the two nuclei are still distinct, the chromosomes can be distinguished as two separate groups derived

from the male and female nuclei respectively. Indications of the first segmentation spindle are also to be observed at this stage as fine staining threads running throughout both nuclei. No definite resting fertilised nucleus is formed.

The spindle, which lies obliquely in the centre of the egg, is at first multipolar in form, and while it is in this condition the chromosomes begin to split longitudinally, but can still be distinguished roughly into two groups.

Only after the formation of four segmentation nuclei do these begin to wander down to the base of the egg. On its way down each nucleus has a distinct sheath of cytoplasmic fibres, but when it reaches the base these become replaced by fine cytoplasmic threads, which run from the nucleus out into the general cytoplasm. These later-formed cytoplasmic threads seem to be connected with the formation of cell walls around the nuclei.

The number of chromosomes in the egg nucleus was determined by counting them in the division which cuts off the ventral canal cell, and was found to be twelve. The same number was also to be found in the nuclei of the cells of the prothallial tissue and of the pollen mother cells. The chromosomes of the first segmentation spindle, on the one occasion on which they could be counted, were exactly twenty-four in number. The chromosomes were also counted in several types of sporophytic tissue; at least twenty-one chromosomes could always be observed; presumably twenty-four are always present.

No centrospheres or centrosomes were to be seen in connection either with fertilisation or with any of the related processes.

“Experiments on Aneroid Barometers at Kew Observatory and their Discussion.” By C. CHREE, Sc.D., LL.D., F.R.S., Superintendent. Received May 5,—Read June 9, 1898.

(Communicated by the Author at the request of the Kew Observatory Committee.)

(Abstract.)

The paper deals with two species of data. The first consists of particulars derived from the records at Kew Observatory as to the errors observed in about 300 aneroid barometers. These had been subjected to the ordinary Kew test, which consists in lowering the pressure to which the aneroid is exposed inch by inch to the lowest point at which verification is desired, and raising the pressure in a corresponding way to its original value. Readings are taken at each inch of pressure during both the fall and the recovery, and a table of corrections is obtained by reference to the corresponding readings of a mercury gauge.

402 *Experiments on Aneroid Barometers and their Discussion.*

The second group of data are the results of special experiments made at Kew Observatory during the last three years. These were intended to link together the phenomena exhibited in the Kew verifications, and to further investigate various points bearing on the usefulness of the certificate hitherto issued to aneroids.

The aneroid is an instrument exhibiting elastic after-effect, frequently in a conspicuous way. When pressure is lowered and then maintained constant, the aneroid's reading continues to fall, and when pressure is restored to its original value, the aneroid reads at first lower than it did originally, but exhibits a gradual tendency to recover. These general facts have of course been long known. The most characteristic features were in fact discussed 30 years ago by Dr. Balfour Stewart, then superintendent of Kew Observatory, who dealt with a considerable mass of experimental material. They have also been the subject of a comparatively recent pamphlet by Mr. Edward Whympers, who gives the results of a number of interesting long period experiments.

The present paper is partly experimental and partly theoretical. It treats of how the differences between the readings with pressure descending and ascending in a normal pressure cycle, such as the Kew test, vary throughout the range, and how the sum of these differences varies from one range to another. It investigates how the error, as pressure is reduced, varies with the rate of fall of pressure (when uniform), how the fall of reading at a low stationary pressure increases with the time, depends on the pressure, and varies with the rate of the previous fall of pressure, and how the recovery after a pressure cycle progresses with the time, and is modified by the nature of the previous pressure changes. The influence of subsidiary stoppages during the fall or rise of pressure is investigated, and experiments are discussed showing the influence of temperature on the various phenomena.

Some of the aneroids employed for the special experiments having been under observation for nearly three years, the opportunity is taken of considering the secular change of zero, and also changes in the elastic and the after-effect properties.

Algebraic and exponential formulæ are obtained by trial for such phenomena as the variation of the differences of the descending and ascending readings throughout a pressure cycle, the dependence of the sum of such differences on the range, the fall of reading at the lowest pressure and the final recovery. A theory, to some extent empirical, is built up, leading to mathematical results, depending on only three arbitrary constants, for the behaviour of an aneroid in the ordinary Kew test over any range. One of these constants varies with the aneroid, but is determined by the observed value of such a quantity as the sum of the differences of the descending and ascending

readings over any convenient range. The other two constants depend on the length of the stoppage at the lowest point of the range, and the relation between the rates of the lowering and the recovery of pressure. The results calculated in this way show a very satisfactory agreement with the Kew verifications.

The investigation being principally intended to increase the usefulness of the existing Kew test, and to show where it is most in need of amplification or amendment, attention is primarily given to the defects of aneroids. It is hoped that the increased knowledge of these defects will enable rules to be framed for the rejection of aneroids, and that in this way it will be made worth while for makers to improve the instrument. The large differences brought to light between different aneroids, show that the means of markedly raising the average are already at the makers' disposal if they choose to utilise their knowledge. The present enquiry also shows clearly how the effects of tentative improvements may best be ascertained. The method of utilising aneroids to the best advantage in determining mountain heights is not formally considered, but a variety of the results should nevertheless be found of immediate service by any traveller of intelligence who has this object in view.

“On the Heat dissipated by a Platinum Surface at High Temperatures.” By J. E. PETAVEL, 1851 Exhibition Scholar. Communicated by LORD RAYLEIGH, F.R.S. Received May 19,—Read June 9, 1898.

(Abstract.)

The first part of the paper refers to the emissivity of a bright platinum surface in air and in other gases.

The temperature measurements are based on the researches of Callendar and Griffiths, confirmed by the recent determinations of Heycock and Neville. To check the calibration of the thermometers at higher temperatures, the melting point of palladium was used. A number of the curves are extended to 1779° C. by a direct measurement of the emissivity of platinum and palladium at their melting points.

The platinum wire, which served at the same time as radiator and thermometer, was 0.112 cm. in diameter. It was placed in the axis of a vertical glass cylinder, which formed the enclosure.

The effects produced by a change in the size, shape, material, and temperature of the enclosure and in the position and diameter of the wire are also studied.

The temperature is expressed in degrees centigrade, and the emissivity in C.G.S. units.

Emissivity of a Bright Platinum Surface in Air, Carbon Dioxide, Hydrogen, Oxygen, and Steam.

Gas contained in the enclosure	Air.			Carbon dioxide.			Hydrogen.			Oxygen.	Steam.
	1	76	228	6	76	228	6	76	228	228	76
Pressure in centimetres of mercury											
Temperature in degrees centigrade.											
200	0.00043	0.00079	0.00110	0.000135	0.000803	0.00113	0.00216	0.00290	0.00373	0.00107	
600	0.00081	0.00125	0.00174	0.000820	0.00134	0.001865	0.00808	0.00413	0.00635	0.001755	0.00078
900	0.00117	0.00173	0.00229	0.001295	0.00191	0.00252	0.00885	0.00503	0.00658	0.002335	0.00225
1200	0.00186	0.00247	0.00287	0.00200	0.00264	0.00333	0.00478	0.00604	0.00793	0.003115	0.00320
1779	..	0.00497	0.00041			

The emissivity is expressed in C.G.S. units. The temperature of the enclosure was kept at about 10° C. The diameter of the radiating wire was 0.112 cm.

An abstract of the values obtained is given in the table (p. 404).

Part II consists of a bolometric study of the radiation emitted by platinum at temperatures ranging from 500° C. to the melting point of the metal. It is shown that for theoretical reasons the true rate of change of the total radiation with temperature lies between the values obtained by measuring the heat lost by the radiating body and those deduced from the readings of any form of bolometer or thermopile.

By comparing the observations of Dr. J. T. Bottomley and Schleiermacher, based on the first method, with those of F. Paschen and of the author, made by the second method, a reliable criterion is obtained by which to test any formula intended to express the law of thermal radiation.

The formulæ of Dulong and Petit, of Stefan, and of Rosetti fail when tested in this manner; whilst Weber's formula, from 400° to 800° C., gives results in close agreement with the true rate of change of total radiation with regard to temperature.

The second part of the paper also contains a description of some points of interest in the design of the bolometer which was used during this work.

Part III refers to the variation of the intrinsic brilliancy of platinum surface with temperature.

The results may be expressed by the following formula:—

$$(t-400) = 889.6 \sqrt[3]{b},$$

where t is the temperature in degrees centigrade, and b the intrinsic brilliancy in candle power per square centimetre. The constant 400 is taken as the temperature limit at which the visible radiation falls to zero.

“On a new Constituent of Atmospheric Air.” By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS. Received June 3—Read June 9, 1898.

This preliminary note is intended to give a very brief account of experiments which have been carried out during the past year to ascertain whether, in addition to nitrogen, oxygen, and argon, there are any gases in air which have escaped observation owing to their being present in very minute quantity. In collaboration with Miss Emily Aston we have found that the nitride of magnesium, resulting from the absorption of nitrogen from atmospheric air, on treatment with water yields only a trace of gas; that gas is hydrogen, and arises from a small quantity of metallic magnesium unconverted into nitride. That the ammonia produced on treatment

with water is pure has already been proved by the fact that Lord Rayleigh found that the nitrogen obtained from it had the normal density. The magnesia, resulting from the nitride, yields only a trace of soluble matter to water, and that consists wholly of hydroxide and carbonate. So far, then, the results have been negative.

Recently, however, owing to the kindness of Dr. W. Hampson, we have been furnished with about 750 cubic centimetres of liquid air, and, on allowing all but 10 cubic centimetres to evaporate away slowly, and collecting the gas from that small residue in a gas-holder, we obtained, after removal of oxygen with metallic copper, and nitrogen with a mixture of pure lime and magnesium dust, followed by exposure to electric sparks in presence of oxygen and caustic soda, 26·2 cubic centimetres of a gas, showing the argon spectrum feebly, and, in addition, a spectrum which has, we believe, not been seen before.

We have not yet succeeded in disentangling the new spectrum completely from the argon spectrum, but it is characterised by two very brilliant lines, one almost identical in position with D_3 , and almost rivalling it in brilliancy. Measurements made by Mr. E. C. C. Baly, with a grating of 14,438 lines to the inch, gave the following numbers, *all four lines being in the field at once* :—

D_1	5895·0
D_2	5889·0
D_3	5875·9
D_4	5867·7

There is also a green line, comparable with the green helium line in intensity, of wave-length 5568·8, and a somewhat weaker green, the wave-length of which is 5560·6.

In order to determine as far as possible which lines belong to the argon spectrum, and which to the new gas, both spectra were examined at the same time with the grating, the first order being employed. The lines which were absent, or very feeble, in argon, have been ascribed to the new gas. Owing to their feeble intensity, the measurements of the wave-lengths which follow must not be credited with the same degree of accuracy as the three already given, but the first three digits may be taken as substantially correct :—

Violet.....	4317	Blue.....	4834
„	4387	„	4909
„	4461	Green	5560·6
„	4671	„	5568·8
Blue	4736	Yellow.....	5829
„	4807	„	5867·7
„	4830	Orange.....	6011

Mr. Baly has kindly undertaken to make a study of the spectrum, which will be published when complete. The figures already given, however, suffice to characterise the gas as a new one.

The approximate density of the gas was determined by weighing it in a bulb of 32.321 cubic centimetres capacity, under a pressure of 521.85 millimetres, and at a temperature of 15.95°. The weight of this quantity was 0.04213 gram. This implies a density of 22.47, that of oxygen being taken as 16. A second determination, after sparking for four hours with oxygen in presence of soda, was made in the same bulb; the pressure was 523.7 millimetres, and the temperature was 16.45°. The weight was 0.04228 gram, which implies the density 22.51.

The wave-length of sound was determined in the gas by the method described in the "Argon" paper. The data are:—

	i.	ii.	iii.
Wave length in air.....	34.17	34.30	34.57
„ „ gas	29.87	30.13	

Calculating by the formula

$$\lambda_{\text{air}}^3 \times \text{density}_{\text{air}} : \lambda_{\text{gas}}^3 \times \text{density}_{\text{gas}} :: \gamma_{\text{air}} : \gamma_{\text{gas}}$$

$$(34.33)^3 \times 14.479 : (30)^3 \times 22.47 :: 1.408 : 1.666,$$

it is seen that, like argon and helium, the new gas is monatomic and therefore an element.

From what has preceded, it may be concluded that the atmosphere contains a hitherto undiscovered gas with a characteristic spectrum, heavier than argon, and less volatile than nitrogen, oxygen, and argon; the ratio of its specific heats would lead to the inference that it is monatomic, and therefore an element. If this conclusion turns out to be well substantiated, we propose to call it "krypton," or "hidden." Its symbol would then be Kr.

It is, of course, impossible to state positively what position in the periodic table this new constituent of our atmosphere will occupy. The number 22.51 must be taken as a minimum density. If we may hazard a conjecture, it is that krypton will turn out to have the density 40, with a corresponding atomic weight 80, and will be found to belong to the helium series, as is, indeed, rendered probable by its withstanding the action of red-hot magnesium and calcium on the one hand, and on the other of oxygen in presence of caustic soda, under the influence of electric sparks. We shall procure a larger supply of the gas, and endeavour to separate it more completely from argon by fractional distillation.

It may be remarked in passing that Messrs. Kayser and Friedländer, who supposed that they had observed D₃ in the argon of the

atmosphere, have probably been misled by the close proximity of the brilliant yellow line of krypton to the helium line.

On the assumption of the truth of Dr. Johnstone Stoney's hypothesis that gases of a higher density than ammonia will be found in our atmosphere, it is by no means improbable that a gas lighter than nitrogen will also be found in air. We have already spent several months in preparation for a search for it, and will be able to state ere long whether the supposition is well founded.

"On the Position of Helium, Argon, and Krypton in the Scheme of Elements." By SIR WILLIAM CROOKES, F.R.S.
Received and Read June 9, 1898.

It has been found difficult to give the elements argon and helium (and I think the same difficulty will exist in respect to the gas krypton) their proper place in the scheme of arrangement of the elements which we owe to the ingenuity and scientific acumen of Newlands, Mendeléeef and others. Some years ago, carrying a little further Professor Emerson Reynold's idea of representing the scheme of elements by a zigzag line, I thought of projecting a scheme in three dimensional space, and exhibited at one of the meetings of the Chemical Society* a model illustrating my views. Since that time, I have re-arranged the positions then assigned to some of the less known elements in accordance with later atomic weight determinations, and thereby made the curve more symmetrical.

Many of the elemental facts can be well explained by supposing the space projection of the scheme of elements to be a spiral. This curve is, however, inadmissible, inasmuch as the curve has to pass through a point neutral as to electricity and chemical energy twice in each cycle. We must therefore adopt some other figure. A figure-of-eight will foreshorten into a zigzag as well as a spiral, and it fulfils every condition of the problem. Such a figure will result from three very simple simultaneous motions. First, an oscillation to and fro (suppose east and west); secondly, an oscillation at right angles to the former (suppose north and south), and thirdly, a motion at right angles to these two (suppose downwards), which, in its simplest form, would be with unvarying velocity.

I take any arbitrary and convenient figure-of-eight, without reference to its exact nature; I divide each of the loops into eight equal parts, and then drop from these points ordinates corresponding to the atomic weights of the first cycle of elements. I have here a model representing this figure projected in space; in it the elements are

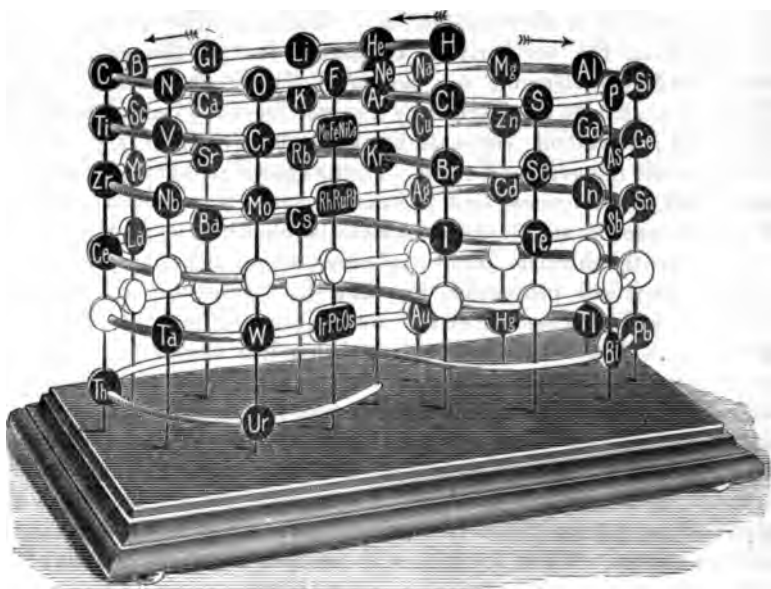
* Presidential Address to the Chemical Society, March 28, 1898.

supposed to follow one another at equal distances along the figure-of-eight spiral, a gap of one division being left at the point of crossing. The vertical height is divided into 240 equal parts on which the atomic weights are plotted, from $H = 1$ to $Ur = 239.59$. Each black disc represents an element, and is accurately on a level with its atomic weight on the vertical scale.

The accompanying figure, photographed from the solid model, illustrates the proposed arrangement. The elements falling one under the other along each of the vertical ordinates, are

H	He	Li	Gl	B	C	N	O	F	Na	Mg	Al	Si	P	S
Cl	Ar	K	Ca	Sc	Ti	V	Cr	Mn·Fe·Ni·Co	Cu	Zn	Ga	Ge	As	Se
Br	Kr	Rb	Sr	Yt	Zr	Nb	Mo	Rh·Ru·Pd	Ag	Cd	In	Sn	Sb	Te
I	—	Cs	Ba	La	Ce	()	()	()	()	()	()	()	()	()
()	—	()	()	()	()	Ta	W	Ir·Pt·Os	Au	Hg	Tl	Pb	Bi	—
—	—	—	—	—	Th	—	Ur	—	—	—	—	—	—	—

The bracketed spaces between cerium and tantalum are probably occupied by elements of the didymium and erbium groups. Their chemical properties are not known with sufficient accuracy to enable their positions to be well defined. They all give coloured absorption spectra and have atomic weights between these limits. Positions marked by a dash (—) are waiting for future discoverers to fill up.



Let me suppose that at the birth of the elements, as we now know them, the action of the *vis generatrix* might be diagrammatically represented by a journey to and fro in cycles along a figure-of-eight

410 *Position of Helium, Argon, and Krypton in the Elements.*

path, while simultaneously time is flowing on, and some circumstance by which the element-forming cause is conditioned (*e.g.*, temperature) is declining; (variations which I have endeavoured to represent by the downward slope.) The result of the first cycle may be represented in the diagram by supposing that the unknown formative cause has scattered along its journey the groupings now called hydrogen, lithium, glucinum, boron, carbon, nitrogen, oxygen, fluorine, sodium, magnesium, aluminium, silicon, phosphorus, sulphur, and chlorine. But the swing of the pendulum is not arrested at the end of the first round. It still proceeds on its journey, and had the conditions remained constant, the next elementary grouping generated would again be lithium, and the original cycle would eternally reappear, producing again and again the same fourteen elements. But the conditions are not quite the same. Those represented by the two mutually rectangular horizontal components of the motion (say chemical and electrical energy) are not materially modified; that to which the vertical component corresponds has lessened, and so, instead of lithium being repeated by lithium, the groupings which form the commencement of the second cycle, are not lithium, but its lineal descendant, potassium.

It is seen that each coil of the lemniscate track crosses the neutral line at lower and lower points. This line is neutral as to electricity, and neutral as to chemical action. Electro-positive elements are generated on the northerly or retreating half of the swing, and electro-negative elements on the southerly or approaching half. Chemical atomicity is governed by distance from the central point of neutrality; monatomic elements being one remove from it, diatomic elements two removes, and so on. Paramagnetic elements congregate to the left of the neutral line, and diamagnetic elements to the right. With few exceptions, all the most metallic elements lie on the north.

Till recently chemists knew no element which had not more or less marked chemical properties, but now by the researches of Lord Rayleigh and Professor Ramsay, we are brought face to face with a group of bodies with apparently no chemical properties, forming an exception to the other chemical elements. I venture to suggest that these elements, helium, argon, and krypton in this scheme naturally fall into their places as they stand on the neutral line. Helium, with an atomic weight of 4, fits into the neutral position between hydrogen and lithium. Argon, with an atomic weight of about 40, as naturally falls into the neutral position between chlorine and potassium. While krypton with an atomic weight of about 80, will find a place between bromine and rubidium.

See how well the analogous elements follow one another in order: C, Ti, and Zr; N and V; Gl, Ca, Sr, and Ba; Li, K, Rb, and Cs; Cl, Br and I; S, Se, and Te; Mg, Zn, Cd, and Hg; P, As, Sb, and Bi; Al, Ga, In, and Tl. The symmetry of these series shows that we are

on the right track. It also shows how many missing elements are waiting for discovery, and it would not now be impossible to emulate the brilliant feat of Mendeléef in the celebrated cases of Eka-silicon and Eka-aluminium. Along the neutral line alone are places for many more bodies, which will probably increase in density and atomic weight until we come to inert bodies in the solid form.

Four groups are seen under one another, each consisting of closely allied elements which Professor Mendeléef has relegated to his 8th family. They congregate round the atomic weight 57, manganese, iron, nickel and cobalt; round the atomic weight 103, ruthenium, rhodium, and palladium; while lower down round atomic weight 195 are congregated osmium, iridium and platinum. These groups are interperiodic because their atomic weights exclude them from the small periods into which the other elements fall; and because their chemical relations with some members of the neighbouring groups show that they are interperiodic in the sense of being formed in transition stages.

[Note, June 22nd, 1898.

Since the above was written, Professor Ramsay and Mr. Travers have discovered two other inert gases accompanying argon in the atmosphere. These are called Neon and Metargon. From data supplied me by Professor Ramsay, it is probable that neon has an atomic weight of about 22, which would bring it into the neutral position between fluorine and sodium. Metargon is said to have an atomic weight of about 40; if so, it shares the third neutral position with argon. I have marked the positions of these new elements on the diagram.]

June 16, 1898.

SIR JOHN EVANS, K.C.B., D.C.L., Treasurer and Vice-President,
in the Chair.

Mr. H. F. Baker, Mr. S. F. Harmer, Mr. Arthur Lister, Lieut.-General C. A. McMahon, the Hon. Charles A. Parsons, Professor T. Preston, Professor E. Waymouth Reid, Dr. Alexander Scott, Mr. A. C. Seward, Mr. W. A. Shenstone, Mr. H. M. Taylor, and Mr. James Wimshurst were admitted into the Society.

The following Papers were read :—

- "Observations on Stomata." By FRANCIS DARWIN, F.R.S.
- II. "Note on the Attenuation and Exaltation of the Virulence of the Organism of Texas Fever." By A. EDINGTON, M.B.
Communicated by Professor T. R. FRASER, F.R.S.
- III. "Mathematical Contributions to the Theory of Evolution. V. On the Reconstruction of the Stature of Prehistoric Races." By Professor KARL PEARSON, F.R.S.
- IV. "On some Expressions for the Radial and Axial Components of the Magnetic Force in the Interior of Solenoids of Circular Cross-section." By C. COLERIDGE FARR. Communicated by Professor H. LAMB, F.R.S.
- V. "On the Source of the Röntgen Rays in Focus Tubes." By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S.
- VI. "On the Companions of Argon." By Professor RAMSAY, F.R.S., and MORRIS W. TRAVERS.
- VII. "Contributions to our Knowledge of the Fucaceæ, their Life-history and Cytology."* By Professor J. B. FARMER and J. LI. WILLIAMS. Communicated by Dr. D. H. SCOTT, F.R.S.
- VIII. "On the Detection and Localisation of Phosphorus in Animal and Vegetable Tissues." By Professor A. B. MACALLUM. Communicated by Professor SHERRINGTON, F.R.S.

* This is the full paper of which the communication entitled "On Fertilisation, and the Segmentation of the Spore, in *Fucus*," read June 18, 1896, and published in the 'Proceedings,' vol: 60, p. 188, is to be regarded as the 'Abstract.'

- IX. "Summary of the principal Results obtained in a Study of the Development of the Tuatara (*Sphenodon punctatum*). By Professor A. DENDY. Communicated by Professor HOWES, F.R.S.

- X. "Tables for the Solution of the Equation

$$\frac{d^2y}{dx^2} + \frac{1}{x} \frac{dy}{dx} - \left(1 + \frac{h^2}{x^2}\right)y = 0."$$

By W. STEADMAN ALDIS, M.A. Communicated by J. J. THOMSON, F.R.S.

- XI. "The Stomodæum, Mesenterial Filaments, and Endoderm of *Xenia*." By J. H. ASHWORTH, B.Sc. Communicated by Professor HICKSON, F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 17th, 1898.

"Observations on Stomata." By FRANCIS DARWIN, F.R.S.

Received May 31,—Read June 16, 1898.

(Abstract.)

The method described depends on the fact that in adult leaves transpiration is stomatal rather than cuticular, so that, other things being equal, the yield of watery vapour depends on the degree to which the stomata are open, and may be used as an index of their condition. In principle, it is the same as the methods of Merget* and Stahl.† These observers used hygroscopic papers impregnated with reagents which change colour according as they are dry or damp, and Stahl, who employed paper soaked in cobalt chloride, has obtained excellent results. In my laboratory I have used, for some years, a hygroscope for demonstrating stomatal transpiration, in which evaporation is indicated by the untwisting of the awn of *Stipa pennata*;‡ my present instrument is of the same general type, but the index is made of "chinese leaf," i.e., shavings of pressed and heated horn.§ If a strip of horn is placed on a dry substance, e.g., the astomatal surface of a leaf, it does not move, but on the stomatal surface, it instantly curves strongly away from the transpiring surface. In the hygroscope the

* 'Comptes Rendus,' 1878.

† 'Bot. Zeitung,' 1894.

‡ Darwin and Acton, 'Practical Physiology of Plants,' 1st edition, 1894.

§ I also use the epidermis of a *Yucca*—a material which I owe to the kindness of Mr. Thiselton-Dyer.

degree of curvature is read off on a graduated quadrant, and in this way a numerical indication of the condition of the stomata is obtained.

The instrument makes no claim to accuracy, but has proved extremely useful when used comparatively to indicate and localise small changes in the transpiration of leaves, and therefore by implication, changes in the condition of the stomata. By observing under the microscope the uninjured leaf of *Caltha palustris*, and comparing the variations in the size of the stomata with the variations in the readings of the hygroscope, it is easy to convince one's self of the value of the method. It must be especially noted that though a fall in the hygroscope readings corresponds with a narrowing of the stomatal opening, it does not follow that zero on the hygroscopic scale means absolute closure of the stomata. This want of sensitiveness has one advantage, namely, that cuticular transpiration has no effect on the horn index, so that any movement of the index must depend on a stomatal transpiration. The hygroscope indicates well the gradual "closure"* of the stomata that occurs as a plucked leaf withers. It is generally stated that marsh and aquatic plants do not close their stomata under these circumstances. I find that, although the phenomenon is much less marked than in terrestrial plants, yet that, in many species, partial closure of the stomata undoubtedly occurs in the aquatic class.

The most interesting fact observed in withering leaves is that in many cases the "closure" of the stoma is preceded by temporary opening, which may occur almost simultaneously with the severance of the leaf from the plant. Thus the hygroscope readings rise at first, and subsequently sink to zero. The interest of this fact is the demonstration of the interaction between the guard cells and the surrounding epidermis. The phenomenon is best seen in plants with milky juice, but is not confined to this class. The preliminary opening of the stomata occurs in the early morning, but not in the evening—a fact which is of importance in relation to the mechanism of the nocturnal closure of the stomata.

A diminution of the stomatal transpiration can also be brought about by compressing the stem of the plant in a vice, a process which is known to diminish the water supply.† The stomatal closure is here probably an adaptive response to the lowering of the water-supply of the leaf, but this is not quite certain.

A series of experiments were made on the comparative effect of moist and dry air, from which it is clear that the stomata "close" before any visible signs of flaccidity occur in the leaf. When leaves are exposed to air dried by H_2SO_4 , "closure" is preceded by a remark-

* I use the word "closure" to mean such a narrowing of the stomatal aperture as corresponds with zero on the hygroscope.

† F. Darwin and R. Phillips, 'Camb. Phil. Soc. Proc.,' 1886.

ably prolonged opening of the stomata—a phenomenon which requires further investigation.

Baranetzky* showed that slight degrees of disturbance affect transpiration. The hygroscopic gives no evidence of increased transpiration when the disturbance is slight. When the plant is violently shaken the leaves become flaccid and the stomata “close,” and in some cases the closure is preceded by increased transpiration, no doubt due to temporary opening of the stomata, induced by the guard cells being released from epidermal pressure before they have lost their own turgor.

N. J. C. Müllert† showed that stomata may be closed by electric stimulation; my experiments show that while a strong shock narrows the stomata, a weaker one opens them, no doubt owing to the temporary loss of epidermal pressure.

Some experiments on poisonous gases and vapours were made. Chloroform and ether slowly “close” the stomata, which finally reopen in a normal atmosphere. Pure CO₂ also slowly closes the stomata.

The hygroscopic is well fitted to demonstrate the fundamental facts in relation to light. The fact that the stomata are widely open in sunshine is well known; the difference between bright and less bright diffused light is not so well known, nor the fact that in dark stormy weather the stomata may be nearly closed by day, even in summer. The effect of difference of illumination is well shown in certain leaves having stomata in both surfaces, e.g., *Iris*, *Narcissus*, and the phyllodes of *Acacia cyclopis*. In these the stomata on the illuminated surfaces are much wider open than on the less brightly illuminated sides; and when the plant is reversed in position in regard to light, the stomata rapidly accommodate themselves to the change in illumination.

The most interesting fact in regard to the effect of artificial darkness is that it is more effectual in producing closure in the afternoon than in the morning; and, conversely, illumination opens closed stomata more readily in the morning than later in the day. These, together with other observations, tend to show a certain amount of inherent periodicity in the nocturnal closure of the stomata. Another fact of interest is that in darkness prolonged for several days the stomata gradually open. This last observation is used in the section on the mechanism of the stoma as an argument against the prevalent view that the stoma closes in darkness, because in the abeyance of assimilation the osmotic material, on which the turgor of the guard cells depends, ceases to be manufactured.

* ‘Bot. Zeitung,’ 1872.

† Pringsheim’s ‘Jahrbücher,’ vol. 8, 1872.

Schellenberger* has striven to uphold this view by showing that in the absence of CO_2 the stomata close as though they were in darkness. My experiments on plants deprived of CO_2 lead to absolutely contrary results, namely, that the stomata remain perfectly open even during prolonged deprivation of CO_2 .

It is a vexed question† whether or no the majority of plants close their stomata at night. My conclusion is that in terrestrial plants (excluding nyctitropic plants) a great majority show some closure at night; the horn hygroscope stands at zero on the stomatal surface of by far the greater number of ordinary plants. On the other hand, the hygroscope shows widely open stomata on most aquatic plants at night. Stahl‡ concludes that nyctitropic plants are remarkable for not closing the stomata at night; this fact I somewhat doubtfully confirm; but the question is not so simple as it seems, owing to the varying behaviour of the stomata at night in different temperatures.

Since the hygroscope gives numerical readings, it is possible to represent graphically the daily opening and closing of the stomata. The curve begins to leave the zero with the morning light; it rises rapidly at first, and afterwards more slowly. In some cases it runs roughly horizontally until a rapid fall begins in the evening. In other cases there is a slow rise up to the highest point, which occurs between 11 A.M. and 3 P.M. The hygroscope generally sinks to zero within an hour after sunset.

The effect of heat has not been fully studied, but enough has been done to confirm previous observers who find that heat opens the stomata. As regards the visible spectrum, I find that the red rays are decidedly most efficient, but I am not able to find any evidence of a secondary maximum in the blue, such as Kohl§ describes.

The biology of the nocturnal closure is a subject which can hardly be discussed in a condensed manner. It is suggested that the gaseous interchange of assimilation may require widely open stomata, whereas respiration may be carried on with comparatively closed apertures. If this is so, the stomata might be to a great extent shut at night, and an economy in the use of water effected, without detriment to metabolism. Observations are given to show that quite another effect is brought about by nocturnal closure. As long as the stomata are open, the transpiring leaf is considerably cooler than the dry-bulb thermometer, but at night it has almost the temperature of the air. In this way a saving of heat is undoubtedly effected—but it is not easy to say whether it is sufficient to be of much

* 'Bot. Zeitung,' 1896.

† Leitgeb, 'Mittheilungen aus dem Bot. Inst. zu Graz,' 1886.

‡ 'Bot. Zeitung,' 1897.

§ 'Beiblatt zur Leopoldina,' 1895.

practical importance to the plant. I am inclined to believe, from Sachs'* experiments on the depletion of leaves, that all saving of heat must be valuable, by preventing the checking of translocation which he observed on cold nights.

The mechanism of the stoma is another subject which does not lend itself to condensed treatment. I have tried to point out that the stoma has been neglected in the modern reorganisation of plant physiology from the point of view of irritability. Some observers insist on the preponderant influence of the guard cells, while Leitgeb in the same way exaggerated the importance of epidermic pressure, whereas the two factors should, as far as possible, be considered as parts of a whole and as correlated rather than opposed in action. I have also attempted to show how the stoma, like other parts of the plant, may be supposed to react adaptively to those signals, which we usually call stimuli. The attempt which I have made to rank the problem among the phenomena of irritability, is very tentative in character. I have ventured to put it forth because I am convinced that it is in this direction that advances will be made.

“Mathematical Contributions to the Theory of Evolution. V.
On the Reconstruction of the Stature of Prehistoric Races.”
By KARL PEARSON, F.R.S., University College, London.
Received June 6,—Read June 16, 1898.

(Abstract).

1. The object of this memoir is to illustrate the general theory by which we may reconstruct from the knowledge of one organ in a fossil or prehistoric race, the dimensions of other organs, when the correlation between organs in existing races of the same species has been ascertained.* The particular illustration chosen is the reconstruction of probable stature from a measurement of the long bones.

Up till quite recently this subject remained in great obscurity, partly on account of absence of theory, and partly for want of trustworthy data.

2. The estimated statures as obtained by Orfila, Topinard or Beddoe, or by use of their methods, differ widely, and those methods have no satisfactory theoretical basis. It was usual to suppose that there was some mean or average ratio of stature to long bone, and even when it was recognised that this ratio varied with the length of the long bone, it was thought sufficient to determine it for two or three separate ranges of stature, and determine its mean value for these ranges by a very limited number of cases.

* ‘Arbeiten,’ 1884.

3. The first stage in advance was taken when Rollet published his measurements made in the Anatomical Theatre at Lyons, of the stature and long bones of 100 corpses. Rollet's attempt to establish ratios on the basis of his measurements is not very satisfactory, but to him belongs the credit of having first provided a respectable, if not large amount of data. Rollet's work was followed by a very able memoir on the reconstruction of stature by Manouvrier. Rejecting about one half Rollet's data, he constructed tables giving the average stature for certain ranges of each of the six long bones, and further what he terms *coefficients moyens ultimes*, for ascertaining the stature corresponding to long bones lying outside the limits of his *tableau barème*. There are many traces in Manouvrier's paper of the old view of a "coefficient" by which the long bone must be multiplied in order to obtain the stature. Beyond this view, it cannot be said to contain any theory, and it suffers from certain marked defects. In the first place, proper allowance does not seem to be made for cartilage and the disappearance of animal matter from the bones. The 2 mm. allowed on each bone appear by no means sufficient. In the second place, Manouvrier does not seem to me to justify his extension of results obtained from the French to very divergent races. He merely remarks that individual variations are greater than ethnic. Even if this extension be made, it must be done with hesitation, and with a full recognition of the assumptions made. Lastly, we may note that the statures obtained from the different long bones by Manouvrier's table for particular races, are often rather widely divergent among themselves, and no attempt is made to account for this divergence.

4. Manouvrier's memoir was rapidly followed by an excellent piece of work from Rahon, who collected measurements of the long bones of a very wide series of local races of man, and reconstructed their stature by aid of Manouvrier's tables. This memoir will remain of first-class importance even if better reconstruction formulæ than those of Manouvrier are adopted. Manouvrier's tables have been used in recent German memoirs, as those of Nitsche and Kollmann on the skeletons of the Row Graves and of the *Schweizersbild*. They are accepted at present as the standard tables for the reconstruction of stature.

5. The present memoir starts with the theory of probability, which the author has already applied to other problems in evolution, and deduces the most probable stature for any combination of the four long bones. It is shown that for a population with normal correlation, the relation between stature and one or more long bones is always linear. A general theorem is proved to show that no linear function of the long bones can give the probable stature with so small a probable error as the regression formula of the theory of probability. From this result the following conclusions are obtained:

(a) No constancy of the ratio stature to long bone is theoretically to be expected, but the ratio of deviation from mean stature to deviation from mean long bone, *i.e.*, the regression coefficient is the quantity, the constancy of which might be expected.

(b) No method of predicting individual stature from the individual long bones, whether one or all are used, can give a result with a less probable error than 2 cm.

(c) For the same length of femur, tibia, and humerus, the stature is shorter the longer the radius. This result has considerable bearing on the relationship of man to the anthropomorphous apes.

6. Formulæ are then obtained for the reconstruction of probable stature as measured :

(a) On corpse, from the lengths of the long bones containing animal matter, and with the cartilages attached. These will possibly be of service for purposes of criminal investigation.

(b) In life, from the lengths of the long bones without cartilages, and free of all animal matter.

Corrections are given for cases in which the femur is measured in the oblique position; the tibia is measured with the spine; and the left, instead of the right, hand members are known.

7. While the formulæ are tested on twenty cases, taken at random from Rollet's data, with fairly satisfactory results, the step from variation within the local race to racial variations is not made without a consideration of the circumstances under which natural selection will modify a regression formula. It is pointed out that the divergence between such regression formulæ really enables us to predict to some extent the nature of the differential selection which has taken place between two local races. To test how far we may safely apply our formulæ to other than French measurements, the stature of the Ainos ♂ and ♀ is reconstructed by means of them from Koganei's measurements of the long bones, and the result is found to be very satisfactory. If French regression formulæ give good results for the Aino, they will give, in all probability, good results for prehistoric European races. At the same time it is most important that material should be obtained for an independent investigation of the regression formulæ for another European race. With a view of illustrating the change in the regression formulæ owing to selection, the anthropomorphous apes are considered, and it is shown that the gorilla, in the regression formulæ for femur and tibia stands much closer to man than either the chimpanzee or orang.

8. The formulæ are applied to reconstruct the stature of Palæolithic man, Neolithic man in France and Britain, of the Dolmen-builders and Guanches, of Round Barrow British, and Row Grave Germans, of Romano-British and Romano-Gauls, of Anglo-Saxons, Franks, mediæval French, and of the Naqada race discovered by Professor

Petrie, and measured by Mr. Warren. The modern populations occupying the same districts of Europe as Palæolithic and Neolithic man appear to be taller, but in the case of both south Germany and France there appears to be a slight, but sensible, decrease of stature since prehistoric times. Modern English do not seem to have decreased in stature since the ancient Anglo-Saxons. In the estimates of stature for the above races, the author differs, in some cases very considerably, from previous writers.

9. Beyond the range of normal population (say from 157 to 175 cm. for ♂), the line of regression ceases to be linear. An attempt is made, such as existing data will allow of, to express the line of regression by the equation to a curve. The constants of this curve are determined for measurements of the four chief long bones, and the results exhibited in a diagram, from which it is possible to deduce the probable stature corresponding to a given length of any long bone by inspection. The prediction of the stature of dwarfs from the curve obtained from the data of giants shows only 2.25 cm. mean error, and must be considered satisfactory. Application is then made of the results to reconstruct the stature of Bushmen, Andamanese, and Akkas. These give sufficiently good results to lead us to believe that a fair estimate can be made of the stature of European neolithic dwarfs.

The memoir concludes with a table of reconstructed statures and sexual ratios.

“The Nature of the Antagonism between Toxins and Antitoxins.” By C. J. MARTIN, M.B., D.Sc. Lond., Acting Professor of Physiology, and THOMAS CHERRY, M.D., M.S. Melb., Demonstrator and Assistant Lecturer in Pathology in the University of Melbourne. Communicated by W. D. HALLIBURTON, F.R.S. Received May 7—Read June 9, 1898.

In the ‘Deutsche Med. Wochenschrift’ for 1894 appeared a controversy on this subject between Behring and Buchner. Behring maintained that the antagonism was of a chemical nature, and that the antitoxin neutralised the toxin much as an alkali neutralised an acid. Buchner, on the other hand, adduced results opposed to this view, pointing to the interpretation that the action was an indirect one, due to the antitoxin operating in some indirect way through the medium of the cells of the organism. Since this controversy many investigations have been made with the object of deciding this fundamental point. At the present time, however, opinion is still divided;

Behring, Ehrlich, and Kanthack being principal exponents of the direct chemical view, whilst Buchner, Roux, and Metchnikoff still maintain that the interaction takes place only through the intervention of some cells in the body.

We will briefly review the more important observations which bear directly upon the subject of our paper.

(1) *Observations favouring the Interpretation that the Action of Antitoxins is indirect.*

Calmette* in 1895 made experiments with the toxin of cobra poison and its antitoxin, which he had recently succeeded in producing. Cobra poison is not apparently attenuated by heating its solutions to 68° C. for ten minutes. The antitoxin is, however, completely destroyed by this treatment. Mixtures of cobra poison and antitoxin which produced no symptoms when injected into a rabbit, killed similar rabbits in a few hours if after the mixture had remained in contact for ten minutes it were heated for another ten minutes to 68° C. before injecting. From his experiments Calmette concluded that the toxin of snake venom does not interact with its antitoxin *in vitro*, but only *in corpore*, and therefore that its action cannot be explained as a simple chemical operation between the two.

Wassermann† found that the toxin produced by the bacillus pyocyaneus was not destroyed by boiling, whereas its antitoxin was. The amount of toxin and antitoxin which neutralised each other was first determined by experiment, then the same quantities and proportions of these substances were allowed to remain in contact and afterwards heated to boiling. The animals receiving an injection of this heated mixture died, whereas the control animals which received an equal dose unheated recovered. From these experiments Wassermann concluded that the toxin of pyocyaneus does not interact with its antitoxin *in vitro*, but only *in corpore*, and therefore that it cannot be explained as a simple chemical operation between the two.

Nikanorow‡ discovered that the precipitate formed by the addition of a 1 per cent. solution of cupric acetate was possessed of antitoxic properties and the filtrate not. A 1 per cent. solution of cupric acetate does not, however, precipitate the toxin. Mixtures of the two could thus be separated by the use of this reagent. Experiments conducted along the lines mentioned in the experiments above led to identical results.

Marenghi§ made some observations with the toxin and antitoxin

* 'Ann. de l'Inst. Pasteur,' 1895, p. 250.

† 'Zeits. f. Hyg.,' 1896, B. 22, p. 263.

‡ 'Wratsch,' 1896, vol. 31, abstracted 'J. Ph. Chem.,' 1896, p. 983.

§ 'Centralbl. f. Bakt.,' 1897, vol. 22, p. 521.

of diphtheria which were identical in principle with those of Calmette and Wassermann. In this case, however, it is the toxin which is destroyed at the lower temperature (60° C. Roux and Yersin), whereas the antitoxic properties still remained after heating the serum to 70° C. Mixtures of the two in such proportions as to cause no symptoms when injected into a guinea-pig were made. After heating such mixture to a temperature sufficient to destroy the toxin, the mixture was discovered now to possess antitoxic properties which could be titrated against a fresh amount of toxin.

2. *Observations favouring the interpretation that the action of Antitoxins is direct.*

The view that the operation is a direct one has always received support from the general truth of the "law of multiples," on which indeed the antitoxin notation has been founded. It is further strengthened by the observations of Kanthack and Ehrlich.

Kanthack,* in 1896, demonstrated that the influence of cobra poison in preventing the coagulation of *shed* blood, observed by Cunningham, was prevented by the previous admixture of some of Calmette's antivenomous serum to the solution of cobra poison.

Ehrlich† found that if a solution of ricin, be added to defibrinated blood (? animal) the corpuscles are precipitated in a clump. A ricin solution of the same strength, but containing a little serum from an animal immunised against ricin, failed to produce this result.

Within the last few days we have received a short account of some experiments by Stephens and Meyers,‡ bearing upon the same point. Cobra poison exercises a hæmolytic action upon blood *in vitro*. After admixture of the poison with antivenomous serum this hæmolytic action was absent. The necessary precaution of making the solution of the venom with saline solution approximately isotonic with blood serum was taken.

Before relating the results of our own experiments we may point out one source of fallacy in the conclusions drawn by Wassermann, Calmette, Nikanorow, and Marengi, viz., that they take no account of the factor, *time*, which may be a very important element in any possible chemical interaction between toxins and antitoxins. Every chemical reaction has a certain definite velocity coefficient, and the rapidity of action under any circumstances where the reacting compounds are in solution depends upon this coefficient, and also upon

* Demonstrated at meeting of Physiol. Soc., October 1896 (not published in Proceedings).

† 'Fortschr. d. Med.,' 1897, No. 2.

‡ "Report of Proceedings of Path. Soc. of London," 'Lancet,' March 5, 1898, p. 644.

the product of the active masses of the reacting bodies present. Temperature will also exercise an important influence.

As we shall later show, the experiments quoted by these observers can easily be repeated and the same results obtained. Nevertheless their conclusions are quite unjustified, and by modification of the factors, *time, temperature, and active masses*, exactly opposite results may also be obtained.

Experimental results obtained by the Authors.

Our experiments have been conducted with the toxin of diphtheria, and one of the constituents of the poison of the Australian tiger snake (*Hoplocephalus curtus*.)

The diphtheria toxin was prepared by cultivating the organisms in broth made from well hung beef, after the method of Spronck.* It was filtered through a sound Pasteur-Chamberland filter, and the toxin strength of the filtrate determined by injection into a series of guinea-pigs. That with which most of our experiments were conducted, had a minimum lethal dose of 0.12 c.c. per kilogram in 48 hours.

The antitoxins used were Behring's No. 1 and serum from the Pasteur Institute, Paris.

The constituent of the venom used was the one which is not destroyed by heating a solution of venom to 90° C. This constituent resembles most closely, if indeed it be not identical with, the principal constituent of cobra poison; and, as shown by one of us,† Calmette's antivenomous serum possesses a small but decided counter-acting action upon it. This action, though unfortunately of little or no practical importance, is sufficient for our present purpose, for in our experiments we could mix comparatively large quantities of the serum with small fatal doses of the venom *in vitro*. Under these circumstances one could easily neutralise the poison.

The antitoxin was the antivenomous serum prepared by the Pasteur Institute at Lille, and bore date November, 1896.

We endeavoured at the outset to determine whether the action of antitoxins upon toxins were chemical or physiological, by a direct physical method. In 1896 one of us‡ published an account of a method of separating substances of large molecular size from those of smaller, in solutions containing both. This method was simply by filtering through a film of gelatin, supported in the wall of a Pasteur-Chamberland filter. The filtration was accomplished by a pressure of 50 atmospheres.

* 'Ann. de l'Inst. Pasteur,' 1895.

† C. J. M., 'Intercolonial Med. Journ. of Australasia,' August, 1897.

‡ C. J. M., 'Journal of Physiol.,' vol. 20, 1896.

A standardised solution of diphtheria toxin was filtered through such a filter. The filtrate was found to contain diphtheria toxin. This filtrate was then tested to ascertain whether it were as toxic as the original solution. As will be seen from the protocols it was somewhat diminished in toxic power (Protocol I).

The antitoxin of diphtheria, as was shown by Brodie,* does not pass through such a filter. When antitoxic serum is filtered through gelatin, the whole of the proteids, and together with them all antitoxic virtue, are absent from the filtrate (Protocol II). As the toxin is not held back by the filter, whereas the antitoxin is, one is provided with a simple physical means of separating them, *provided they have not reacted upon one another*.

We mixed a solution of toxin containing eight fatal doses per kilogram of guinea-pig in each c.c., with sufficient Behring's antitoxin to more than completely neutralise all the toxin. This mixture was allowed to remain in contact at 30° C. for two hours, and then filtered through the gelatin filter. Varying quantities of the filtrate were injected into guinea-pigs up to nearly 4 c.c. per kilogram of body weight, that is a quantity originally containing 32 fatal doses. The filtrate was quite innocent. The guinea-pigs suffered no inconvenience, and gained weight while under observation in small cages. The injections produced no local œdema.

If the toxin had remained unaffected beside the antitoxin there was nothing to prevent it passing through the filter in virtue of its relatively small molecular size. As, however, it did not do so, we can only conclude that it had entered into some sort of chemical relationship with the relatively large molecules of the anti-toxin during their sojourn together prior to filtration.

Having obtained results so definite, and in apparent contradiction to those of the authors quoted in the beginning of this paper, we next experimented with snake venom in order to repeat Calmette's† observations.

We took a series of rabbits (Protocol V) and injected them with mixtures containing one constituent of the venom of *Hoplocæphalus curtus* and Calmette's antivenomous serum. On reference to the protocols of this series of experiments it is seen that 2 c.c. of this sample of serum was sufficient to counteract an amount of the poison contained in '0002 gram of the dried venom. This amount killed control rabbits in about eight hours (Protocol IV).

In some of the experiments this amount of venom and serum was allowed to remain in contact for fifteen minutes at the laboratory temperature (21° C.) and then heated to 68° C. for ten minutes to destroy the antitoxin. In Calmette's experiments the rabbits

* 'Journ. of Path.,' 1897, p. 460.

† Calmette, *loc. cit.*

injected with this heated mixture died, whereas the controls injected with the mixture which had not been heated lived. From this he concluded that the serum and venom were merely existing side by side, and had not re-acted upon one another. In our experiments, on the contrary, the rabbits injected with the heated and unheated mixtures of venom and serum alike lived, nor did any of them suffer from symptoms such as loss of appetite, loss of weight, or diminished temperature. The only conclusion to be drawn from these experiments is that during the time which elapsed between the mixture of the venom and serum the latter had acted upon the former, so that there was no longer a fatal dose of venom present. The protocols will show that heating for ten minutes to 68° C. has no influence upon the venom. (P. VI, experiment 4.)

These results, while they lead to results in entire agreement with those drawn from the filtration experiments with diphtheria toxin and antitoxin, are diametrically opposed to the results obtained by Calmette. As the experiments are so simple as not to leave any possibility of experimental error, we turned our attention to any existing difference in the conditions under which Calmette and ourselves worked. As previously pointed out, Calmette absolutely neglected the possible influence of time, temperature, and the relative proportions of the active masses of the toxin and antitoxin present in his mixture. Up to the present we have investigated the value of the factors, time, and proportion of active masses, and have shown that these are most important. Indeed, by altering either the one or the other we can produce results which, if these factors be neglected, would lead to diametrically opposite conclusions.

The toxin and antitoxin of this venom are both of great molecular size and complexity. The former is a deuterio-albumose and the latter probably a globulin,* or at any rate its molecular size is of the same order. *A priori* one would expect the velocity coefficient of any reaction between such complex molecules to be a high one, and in addition, from their great molecular weight, the solution will contain relatively few molecules: so that it is not surprising that any chemical operation in which they are concerned should occupy a very appreciable time.

The value of the factors time and influence of proportion of active masses will be best seen in reference to the table below, which is compiled from Protocols VII, VIII, IX, and represents the results of twenty-one experiments. On reading along any horizontal line will be seen the influence upon the result of the time during which the toxin and antitoxin were allowed to operate upon each other, with proportion of active masses constant. On reading any vertical line the influence of varying proportions of active masses with time

* Brodie, *loc. cit.*

of operation constant is indicated. The thick line separates off the fatal results from those in which the rabbits lived. All other factors were kept constant. The solutions were mixed in the varying proportions and stood at laboratory temperature ($20-23^{\circ}\text{C}.$). At stated intervals, by a stop watch, portions were pipetted off, and the reaction terminated by rapidly raising the temperature to $68^{\circ}\text{C}.$ in a water bath. They were kept at this temperature for 10 minutes, cooled, and kept for injection.

Proportion of toxin to antitoxin per kilo.		Control venom only.	Time allowed for interaction of toxin and antitoxin, temp. $20-23^{\circ}\text{C}.$					
Anti-toxin.	Toxin.		2 mins.	5 mins.	10 mins.	15 mins.	30 mins.	Injected unheated ∞ mins.
1 c.c.	2 fatal doses.	Died 15 hours.	Lived (very ill for 2 days)	Lived (ill 1 day).	Lived (no symptoms).	Lived (no symptoms).	Lived (no symptoms).	Lived (no symptoms).
1 c.c.	3 fatal doses.	Died 12 hours.	Died 20 hours.	Died 28 hours.	Lived (ill 2 days).	Lived (ill 1 day).	Lived (no symptoms).	Lived (no symptoms).
1 c.c.	4 fatal doses.	Died 9 hours.	Died 13 hours.	Died 15 hours.	Died 23 hours.	Lived (very ill 2 days).	Lived (no symptoms).	Lived (no symptoms).

In our experiments with diphtheria we allowed abundance of time, 2 hours, for the reaction between the toxin and antitoxin to take place. The surplus of antitoxin was also large, so that the active masses were considerable and the temperature was favourable, viz. $30^{\circ}\text{C}.$ (Protocol III). We have not yet determined the influence of temperature upon the rapidity of the reaction, but our results so far seem sufficiently conclusive to decide the question and leave no room for doubt that the antagonism between the toxins of diphtheria and snake venom and their relative antitoxin is due to a direct chemical action which takes place between them. Further, that the opposite conclusion come to by Calmette, and presumably those of Wassermann, Nikanarow, and Marengi were due to their disregard of the value of time as a factor in such chemical action.

Protocols.

A. EXPERIMENTS WITH DIPHTHERIA TOXIN.

I. *Experiments to ascertain whether the Toxin of Diphtheria passes through a Gelatin Filter.*

The toxins were prepared by the method stated above and filtered through a Pasteur-Chamberland filter to free them from bacilli. The minimal fatal dose calculated for one kilogram weight of guinea-pig was 0·12 c.c. of toxin No. 1, and 0·6 c.c. of toxin No. 2. Portions of each of these toxins were filtered through gelatin and afterwards injected into guinea-pigs. As seen below the filtrates in each case contained toxin, but in less amount than the original solutions.*

Toxin No. 1.

	Amount injected.	Weight of animal in grams.	Result.
Before filtering through gelatin.....	0·25 c.c.	562	Died under 36 hours.
" " "	0·5 "	540	" "
" " "	1·0 "	655	" "
After filtering through gelatin.....	0·12 "	187	" in 41 hours.
" " "	0·24 "	176	" in 30 hours.

Toxin No. 2.

	Amount injected.	Weight of animal in grams.	Result.
Before filtering through gelatin.....	0·5 c.c.	377	Died in 56 hours.
After filtering through gelatin.....	0·5 "	275	" 72 hours.

* This small diminution in toxic power by filtration may possibly be due to the action of oxygen at high tension (50 atmospheres of compressed air) or to the size of the toxin molecule being of the order of the molecular size of albumoses. Albumoses, as shown by one of us ('J. Physiol.,' 1896), pass through gelatin, but less readily than water, and the filtrate is accordingly less concentrated than the original solution.

II. *Experiments to confirm Brodie's* statement that the Antitoxin of Diphtheria does not pass through a Gelatin Filter.*

0.5 c.c. of Pasteur Institute antitoxin was mixed with 1 c.c. of toxin No. 1 (=8 fatal doses per kilo.), and injected into a guinea-pig weighing 260 grams.

The animal remained well and gained 26 grams during the four days it was kept under observation. The same sample of antitoxin was passed through the filter. Of the filtrate 0.6 c.c. was mixed with 0.6 c.c. of the same toxin and injected into a guinea-pig weighing 163 grams. The animal died in 22 hours.

III. *Experiments to show that when Diphtheria Toxin is mixed with Diphtheria Antitoxin in sufficient quantity, and allowed to remain in contact for a sufficient time, the filtrate which has passed through a Gelatin Filter is free from Toxin.*

60 c.c. of toxin No. 1 (containing approximately 500 lethal doses per kilogram) was mixed with 2.5 c.c. of Behring's antitoxin (= 600 units). The two were well mixed and allowed to stand at 30° C. for two hours before filtration.

The filtrate was injected subcutaneously into guinea-pigs as under:—

Weight in grams.	Amount injected.	Amount per kilo. of body weight.	Result.
400	1.0 c.c.	2.5 c.c. = 20 fatal doses	No symptoms.
340	1.25 "	3.6 c.c. = 30 "	" "
318	1.25 "	3.9 c.c. = 32 "	" "

The animals were absolutely unaffected. They never failed in appetite, nor was there any local oedema.

B. EXPERIMENTS WITH SNAKE VENOM.

IV. *Experiments to determine the Minimal Fatal Dose of the Poison used.*

The venom employed was that of *Hoplocephalus curtus*. This had been procured free from admixture with saliva by making the reptiles bite into a watch-glass covered with thin rubber sheeting. The liquid poison was rapidly dried at ordinary temperatures (15—20° C.) over calcium chloride, powdered, and stored in a stoppered bottle.

* *Loc. cit.*

All weights of venom mentioned below refer to this dried venom. About 2—3 milligrams were weighed out for each experiment and dissolved in 0·9 per cent. NaCl solution, so that 1 c.c. contained 0·0001 gram of dried venom.

The solution was then heated momentarily to 90° C. in order to destroy one of the poisonous constituents of the venom of this snake, a proteid which coagulates at 85° C.* This was done because Calmette's serum possesses little or no immunising action against this constituent.†

In all our experiments the same sample of venom was used, and it was treated in the way described above. The injections were always made subcutaneously into the flank.

Animal.	Weight in grams.	Amount of original venom per kilo. of body weight in grams.	Result.
Rabbit. . . .	1360	0·0004	Died in 5 hours.
"	1020	0·0002	" 8 "
"	910	0·0002	" 9 "
"	1250	0·00016	" 10 "
"	1705	0·00015	" 10 "
"	1250	0·0001	" 10 "
"	1240	0·00008	" 12 "
"	1030	0·000075	" 10 "
"	1220	0·00005	" 16 "
"	1370	0·000036	" 24 "
"	1300	0·00003	" 48 "
"	1380	0·00003	Lived. Very ill 3 days.
"	1820	0·0000275	Died in 24 hours.
"	1430	0·000025	" 3 days.
"	1140	0·00002	Lived. Very ill 3 days.
"	1300	0·00002	" "

From the above series it appears that 0·000025 gram per kilo. of body weight is about the lowest fatal dose. In the present paper, in speaking of so many fatal doses, this has been taken as the unit.

V. Experiments to ascertain the value of Calmette's Serum in counter-acting the Poison after it had been deprived by Heat of One Constituent.

The solution of venom was prepared as in Series III. Calmette's serum bore date November, 1896. The two were mixed together in varying proportion, as stated below, and allowed to remain at

* C. J. M., 'Proc. Roy. Soc., N.S.W.,' August, 1896.

† C. J. M., 'Intercol. Med. J.,' August, 1897.

laboratory temperature (23° C.) for fifteen minutes. They were injected subcutaneously in amounts corresponding to the body weight of the animal.

Animal.	Weight in grams.	Amount of venom in grams per kilo.	Amount of serum in c.c. per kilo.	Result.
		= 16 fatal doses.		
Rabbit....	1350	0·0004	None	Died 5 hours.
"	1370	0·0004	0·25	" 6 "
"	1330	0·0004	0·5	" 10 "
"	1370	0·0004	1·0	" 12 "
"	1375	0·0004	1·5	" 16 "
		= 8 fatal doses.		
"	1240	0·0002	1·0	" 38 "
"	1220	0·0002	1·5	Lived. Ill 2 days.
"	690	0·0002	2·0	" No symptoms.
"	1380	0·0002	3·0	" " "

2 c.c. of the serum completely counteracts 0·0002 gram of the venom deprived of one of its constituents.

VI. *Experiments to determine whether mixture of venom and serum (in such proportions as to completely deprive the former of any toxic properties), regained toxic properties after destroying the antitoxin by heating to 68° C. per 10 minutes, subsequent to admixture.*

The venom solution and serum were mixed in the proportion of 1 c.c. of serum to every 0·0001 gram of venom. This proportion was found by the previous series of experiments to be adequate. The two solutions were mixed together, and allowed to remain 30 minutes at a temperature of 23°—24° C., after which the mixture was heated to 68° C. for 10 minutes to destroy the antitoxin. They were then cooled and injected subcutaneously in varying quantities per kilogram weight of the animal. The injections had no effect upon the animals, although they contained originally eight fatal doses of venom. This must therefore have been neutralised by the antitoxin during the time which elapsed before its destruction by heat.

Animal.	Weight in grams.	Amount of venom per kilo.	Amount of serum per kilo.	Result.
i. Rabbit...	1140	0·0002	3 c.c.	} Lived. No symptoms. Died in 9 hours. " 8½ "
ii. " ...	710	0·0002	2 "	
iii. " ...	1210	0·0003	3 "	
*iv. " ...	1020	0·0002	None	
v. " ...	1160	0·0002	None	

VII. Experiments to determine the influence of variations in the time during which venom, and antivenomous serum, operated upon one another.

In this series of experiments, the same samples of venom and serum were employed as before. The relative proportions of the two were kept constant. The venom solution and serum were mixed together in the proportion of 0·0001 gram of venom to 1 c.c. of the serum and well stirred. After they had remained in contact at the temperature of the laboratory (21° C.) for times varying from 2 to 30 minutes, portions were pipetted off, and rapidly raised to 68° C., at which temperature they were kept for 10 minutes. The portions were then rapidly cooled, and injected subcutaneously in quantities proportionate to the body weights of the animals. Amounts of venom and of serum equivalent to 0·0001 gram and 1 c.c. respectively per kilogram of body weight were injected in each case, except the control, when this quantity of venom was employed.

Animal.	Weight in grams.	Time during which venom and serum were in contact before heating.	Results.
i. Rabbit...	1370	Venom only (control)	Died in 9 hours.
ii. "	1320	2 minutes	" 13 "
iii. "	1340	5 "	" 15 "
iv. "	1400	10 "	" 23 "
v. "	1220	15 "	Lived. Very ill for 2 days.
vi. "	1160	30 "	" No symptoms.
vii. "	890	Not heated at all	" " "

* In Experiment No. IV no serum was mixed with the venom, but the venom solution was heated alone to 68° C. for 10 minutes to show that this treatment has no influence upon it.

VIII.—*Similar to VII, except that the preponderance of Antitoxin was greater, viz., 0·000075 gram of venom and 1 c.c. of serum per kilogram of body weight in each case.*

Animal.	Weight in grams.	Time during which venom and serum were left in contact before heating.	Results.
i. Rabbit....	1026	Venom only (control)	Died in 12 hours.
ii. "	1190	2 minutes	" 20 "
iii. "	1130	5 "	" 28 "
iv. "	1060	10 "	Lived. Very ill 2 days.
v. "	1250	15 "	" Ill 1 day.
vi. "	1210	30 "	" No symptoms.
vii. "	1070	Not heated at all	" "

IX.—*Similar to VII and VIII, but the preponderance of Serum is still greater, viz., 0·00005 gram of venom and 1 c.c. of serum per kilogram of body weight in each case.*

Animal.	Weight in grams.	Time during which venom and serum were in contact before heating.	Result.
i. Rabbit....	1070	Venom only (control)	Died in 15 hours.
ii. "	1200	2 minutes	Lived. Very ill for 2 days.
iii. "	1170	5 "	" Off feed 1 day.
iv. "	1130	10 "	" No symptoms.
v. "	1030	15 "	" "
vi. "	1420	30 "	" "
vii. "	1050	Not heated at all	" "

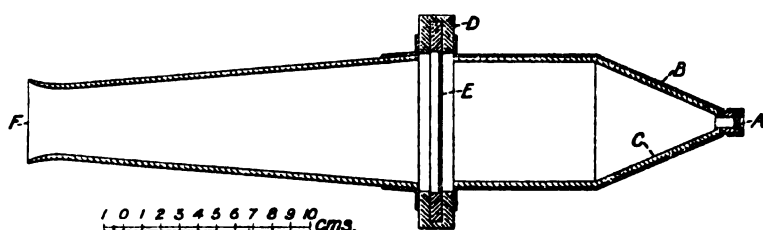
The expenses involved in the foregoing research were in part defrayed from a grant made by the Government Grant Committee of the Royal Society.

"On the Source of the Röntgen Rays in Focus Tubes." By ALAN A. CAMPBELL SWINTON. Communicated by Lord KELVIN, F.R.S. Received June 7,—Read June 16, 1898.

The writer has already described ("Some new Studies in Cathode and Röntgen Radiations," a discourse given at the Royal Institution on February 4, 1898) how he has found it possible to study by means of pin-hole photography the active area on the anti-cathode of a focus tube from which the Röntgen rays proceed.

By means of a special camera he has now been able to make further investigations. The camera is illustrated in fig. 1, where A is the pinhole in a removable lead disc secured by a brass cap to the brass cone B, which is lined with thick lead so as to be opaque to the Röntgen rays. D is a framework into which slides either the fluorescent screen E, or a carrier containing a sensitive plate should photographs be required. F is an observation tube for use with the fluorescent screen. It is made of insulating material to avoid danger of shocks.

FIG. 1.

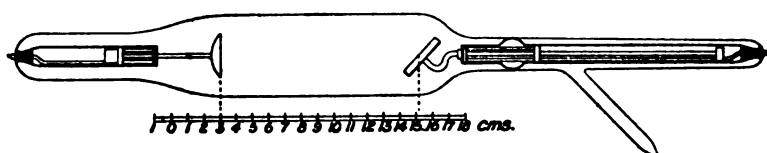


With this apparatus directed at the anti-cathode of a focus tube, it is easy with the fluorescent screen in place to take accurate note of the image of the active anti-cathode area which appears on the screen, and to observe the variations in form, dimensions, and brilliancy that take place under varying conditions. Similarly by replacing the fluorescent screen by a photographic plate in a black paper envelope, the Röntgen ray image can be photographed. Exposures, varying from one to thirty minutes, according to conditions, are found sufficient to impress upon the plate any effect that can be seen directly with the screen. It has not, however, been found possible, even with very prolonged exposures, to photograph anything not directly visible with the screen, and having regard to the difficulties of maintaining the vacuum and other conditions constant for any considerable length of time, the method of direct observation seems generally to be best and most convenient. For direct observation, rather a large pin-hole, say about 2 mm. in diameter, gives the best results; for photography about half this diameter is preferable, as it gives sharper images.

The writer has made numerous observations and photographs with this apparatus, both with focus tubes of the ordinary pattern, and also with a special tube in which both the cathode and also the anti-cathode (which in addition acted as anode) were independently adjustable along the axis of the tube, so that the distance between them could be varied from a minimum of 4 to a maximum of 14 cm. This special tube is illustrated in fig. 2, and during the

observations it was connected to a mercury pump, so that the degree of exhaustion could be varied as desired.

FIG. 2.



The following are the main effects observed.

(1) When the anti-cathode intersects the cathode stream at the focus, the dimensions of the active area are independent of the degree of exhaustion. For all other positions beyond the focus it is larger the lower the exhaustion and *vice versa*. These observations are of course only possible between the limits of exhaustion with which Röntgen rays are produced.

(2) When the anti-cathode intersects the cathode stream beyond the focus, the active area is larger the greater the distance between cathode and anti-cathode. For instance, with the tube illustrated in fig. 2, exhausted to a good Röntgen ray vacuum, it was found that the active area gradually increased from about 0.15 cm. diameter with 4 cm. distance between cathode and anti-cathode up to about 2.3 cm. diameter as the distance was gradually increased to 14 cm. The increase is less the higher the vacua, but is always very considerable.

(3) When the anti-cathode intersects the cathode stream considerably beyond the focus, the active area is found to consist of a well defined and very intense central nucleus, surrounded by a much fainter but quite appreciable halo. Both of these increase in size as the distance between cathode and anti-cathode is increased.

In some cases the halo consists of a well marked hollow ring with a dark space between it and the central nucleus. In other cases two distinct concentric rings are visible surrounding the nucleus. Moreover, the nucleus itself, when very large, shows distinct signs of being made up of one or more concentric rings, sometimes with a still smaller nucleus within them. These observations correspond with and amplify what the writer has already noticed by direct observation of the visible luminescence of a carbon screen arranged to intersect the cathode stream.*

(4) With an anti-cathode inclined at an angle of 45° to the axis of the conical cathode stream, it is found that those portions of the stream which impinge most normally upon the anti-cathode

* 'Roy. Soc. Proc.,' vol. 61, pp. 81—84.

surface are considerably the most efficient in producing Röntgen rays. Similarly those portions of the stream that impinge on the anti-cathode surface very much on the slant are correspondingly ineffective in producing Röntgen rays.

(5) At the degrees of exhaustion most suitable for producing Röntgen rays, and with concave cathodes of the usual dimensions, the cathode stream proceeds almost entirely from a small central portion of the cathode surface, the remaining portion of the surface being apparently practically inoperative. That this is so was very conclusively established by photographs taken with the tube shown in fig. 2. In the manufacture or subsequent exhaustion of this tube three very minute fragments of glass by some means attached themselves on to the concave surface of the aluminium cathode, and remained fixed there during the experiments. The cathode itself was 29 mm. diameter, and the radial distances of the three glass fragments from the centre were respectively about 9 mm., 4 mm., and 2.5 mm. In all the pin-hole photographs of the anti-cathode of this tube in which the enlargement of the active area was sufficient, the shadows of the two glass fragments nearest to the centre of the cathode are clearly visible, while in none of them is there any appearance of the third and outer fragment. It, therefore, is evident that the whole of the cathode stream that was effective in producing Röntgen rays came from an area of the cathode surface less than 18 mm. diameter, or less than two-thirds of the full diameter of the cathode. Further, in each case the shadows of the two inner glass fragments appeared outside of the central nucleus, showing that the whole of the more intense portion of the cathode stream proceeded from a portion of the cathode surface less than 5 mm. in diameter. These results confirm the writer's observations made with carbon cathodes.*

(6) The different portions of the cathode stream proceeding from different portions of the cathode, cross at the focus and diverge in a cone that retains any special characteristics of the convergent cone. The relative positions of the two inner glass fragments on the cathode, and the positions and enlargement of their shadows on the anti-cathode for different distances between the latter and the cathode, were found to show this very clearly.

(7) Though by far the greater portion of, the Röntgen rays given by a focus tube proceed from the active anti-cathode area, still, a very appreciable quantity is also given off by all those portions of the glass of the tube that show the green fluorescence.

Using a somewhat large pin-hole, this is easily observed by turning the tube so that the more powerful rays from the anti-cathode cannot reach the pin-hole, when a Röntgen ray image of the whole of the

* 'Roy. Soc. Proc.,' vol. 61, pp. 92—93.

fluorescent portions of the glass of the tube can be distinctly seen. Further, it is noticeable that that portion of the glass that shows the brightest fluorescence, *i.e.*, that part which lies in the path in which cathode rays would be reflected from the anti-cathode surface were they reflected according to the law of equal angles of incidence and reflection—gives off the most Röntgen rays, while those portions of the glass that show no fluorescence do not give off any Röntgen rays. The conclusion appears obvious that whatever produces the one also produces the other, but as has been pointed out by Professor S. P. Thompson* and others, the fluorescence is not due to the direct stream of rays from the cathode, which cannot reach portions of the glass that show fluorescence, but to some description of radiation that proceeds from the surface of the anti-cathode that faces the cathode. In the paper above referred to Professor Thompson calls these radiations “para-cathodic rays,” stating that they differ from the Röntgen rays in respect of their power of penetration, and in their capacity of being electrostatically and magnetically deflectable. In these respects the writer’s experiments confirm those of Professor Thompson, but when the latter goes on to differentiate these rays from ordinary cathode rays, on account of their not exciting Röntgen rays where they impinge on a solid surface, the writer is unable to agree, for, as above stated, these rays do excite Röntgen rays where they impinge upon the glass walls of the tube; as mentioned, however, they do this only to an extent that is relatively very feeble, and so far as the author knows only discernable by the pin-hole method of observation, which no doubt explains Professor Thompson’s failure to observe the effect. The “para-cathodic” radiations in question do not, however, appear to be ordinary cathode rays. In the first place they do not proceed directly from the cathode, but only from the surface of the anti-cathode that faces the latter. Secondly, they do not appear to be negatively but positively charged, as can be ascertained by means of an exploring pole connected with an electroscope. The writer suggests that, assuming the correctness of the Crookes theory of the nature of the cathode rays, these “para-cathodic” rays may very probably consist of cathode ray particles which, having struck the anti-cathode, and having thus given up their negative charges and acquired positive charges, rebound, both by reason of their elasticity and also by repulsion from the anti-cathode. Perhaps owing to the comparative roughness of the anti-cathode surface, they fly off to some extent in all available directions, but they do so especially in that direction which the law of equal angles of incidence and reflection requires. It also appears very possible that these “paracathodic” rays are identical with the positively electrified streams proceeding from the anode, which the writer has

* See ‘*Phil. Trans.*,’ A., vol. 190, pp. 471—490.

investigated by means of radiometer mill wheels, recently described in a paper to the Physical Society.

In any case, it seems clear that in the tubes observed and photographed with the pin-hole camera, the Röntgen rays given off by certain portions of the fluorescent glass are not originated by the impact of an ordinary cathode stream, but apparently by the impact of positively charged streams proceeding from the anti-cathode.

The writer is greatly indebted to Mr. J. C. M. Stanton and Mr. H. Tyson Wolff, for the construction of the apparatus described, as also for valuable assistance in the carrying out of the experiments.

“On the Companions of Argon.” By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS. Received June 13,—Read June 16, 1898.

For many months past we have been engaged in preparing a large quantity of argon from atmospheric air by absorbing the oxygen with red-hot copper, and the nitrogen with magnesium. The amount we have at our disposal is some 18 litres. It will be remembered that one of us, in conjunction with Dr. Norman Collie, attempted to separate argon into light and heavy portions by means of diffusion, and, although there was a slight difference* in density between the light and the heavy portions, yet we thought the difference too slight to warrant the conclusion that argon is a mixture. But our experience with helium taught us that it is a matter of the greatest difficulty to separate a very small portion of a heavy gas from a large admixture of a light gas; and it therefore appeared advisable to re-investigate argon, with the view of ascertaining whether it is indeed complex.

In the meantime, Dr. Hampson had placed at our disposal his resources for preparing large quantities of liquid air, and it was a simple matter to liquify the argon which we had obtained by causing the liquid air to boil under reduced pressure. By means of a two-way stopcock the argon was allowed to enter a small bulb, cooled by liquid air, after passing through purifying reagents. The two-way stopcock was connected with mercury gas-holders, as well as with a Töpler pump, by means of which any part of the apparatus could be thoroughly exhausted. The argon separated as a liquid, but at the same time a considerable quantity of solid was observed to separate partially round the sides of the tube, and partially below the

* Density of lighter portion, 19.93; of heavier portion, 20.01, ‘*Roy. Soc. Proc.*,’ vol. 60, p. 206.

surface of the liquid. After about 13 or 14 litres of the argon had been condensed, the stopcock was closed, and the temperature was kept low for some minutes in order to establish a condition of equilibrium between the liquid and vapour. In the meantime, the connecting tubes were exhausted and two fractions of gas were taken off by lowering the mercury reservoirs, each fraction consisting of about 50 or 60 cubic cm. These fractions should contain the light gas. In a previous experiment of the same kind, a small fraction of the light gas had been separated, and was found to have the density 17·2. The pressure of the air was now allowed to rise, and the argon distilled away into a separate gas-holder. The white solid which had condensed in the upper portion of the bulb did not appear to evaporate quickly, and that portion which had separated in the liquid did not perceptibly diminish in amount. Towards the end, when almost all the air had boiled away, the last portions of the liquid evaporated slowly, and when the remaining liquid was only sufficient to cover the solid, the bulb was placed in connection with the Töpler pump, and the exhaustion continued until the liquid had entirely disappeared. Only the solid now remained, and the pressure of the gas in the apparatus was only a few millimetres. The bulb was now placed in connection with mercury gas-holders, and the reservoirs were lowered. The solid volatilised very slowly, and was collected in two fractions, each of about 70 or 80 cubic cm. Before the second fraction had been taken off, the air had entirely boiled away, and the jacketing tube had been removed. After about a minute, on wiping off the coating of snow with the finger, the solid was seen to melt, and volatilise into the gas-holder.

The first fraction of gas was mixed with oxygen, and sparked over soda. After removal of the oxygen with phosphorus it was introduced into a vacuum-tube, and the spectrum examined. It was characterised by a number of bright red lines, among which one was particularly brilliant, and a brilliant yellow line, while the green and the blue lines were numerous, but comparatively inconspicuous. The wave-length of the yellow line, measured by Mr. Baly, was 5849·6, with a second-order grating spectrum. It is, therefore, not identical with sodium, helium, or krypton, all of which equal it in intensity. The wave-lengths of these lines are as follows:—

Na (D_1)	5895·0
Na (D_2)	5889·0
He (D_3)	5875·9
Kr (D_4)	5866·5
Ne (D_5)	5849·6

The density of this gas, which we propose to name "neon"

(new), was next determined. A bulb of 32.35 cubic cm. capacity was filled with this sample of neon at 612.4 mm. pressure, and at a temperature of 19.92° it weighed 0.03184 gram.

Density of neon 14.67.

This number approaches to what we had hoped to obtain. In order to bring neon into its position in the periodic table, a density of 10 or 11 is required. Assuming the density of argon to be 20, and that of pure neon 10, the sample contains 53.3 per cent. of the new gas. If the density of neon be taken as 11, there is 59.2 per cent. present in the sample. The fact that the density has decreased from 17.2 to 14.7 shows that there is a considerable likelihood that the gas can be further purified by fractionation.*

That this gas is a new one is sufficiently proved, not merely by the novelty of its spectrum and by its low density, but also by its behaviour in a vacuum-tube. Unlike helium, argon, and krypton, it is rapidly absorbed by the red-hot aluminium electrodes of a vacuum-tube, and the appearance of the tube changes, as pressure falls, from fiery red to a most brilliant orange, which is seen in no other gas.

We now come to the gas obtained by the volatilisation of the white solid which remained after the liquid argon had boiled away.

When introduced into a vacuum-tube it showed a very complex spectrum, totally differing from that of argon, while resembling it in general character. With low dispersion it appeared to be a banded spectrum, but with a grating, single bright lines appear, about equidistant throughout the spectrum, the intermediate space being filled with many dim, yet well-defined lines. Mr. Baly has measured the bright lines, with the following results. The nearest argon lines, as measured by Mr. Crookes, are placed in brackets:—

Reds very feeble, not measured.		
First green band, first bright line	5632.5	(5651 : 5619)
" second " 	5583.0	(5619 : 5567)
" third " 	5537.0	(5557 : 5320)
Second green band, first bright line ...	5163.0	(5165)
" second " 	5126.5	(5165 : 5065) brilliant.
First blue band, first bright line	4733.5	(4879)
" second " 	4711.5	(4701)
Second blue band, first bright line....	4604.5	(4629 : 4594)
Third blue band (first order)	4314.0	(4333 : 4300)
Fourth blue band (second order)	4213.5	(4251 : 4201)
Fifth blue band (first order), about....	3878	(3904 : 3835)

The red pair of argon lines were faintly visible in the spectrum.

The density of this gas was determined with the following

* June 16th. After fractionation of the neon, the density of the lightest sample had decreased to 13.7.

results:—A globe of 32·35 c.c. capacity, filled at a pressure of 765·0 mm., and at the temperature 17·43°, weighed 0·05442 gram. The density is therefore 19·87. A second determination, made after sparking, gave no different result. This density does not sensibly differ from that of argon.

Thinking that the gas might possibly prove to be diatomic, we proceeded to determine the ratio of specific heats:—

Wave-length of sound in air	34·18
" " gas	31·68
Ratio for air	1·408
" gas	1·660

The gas is therefore monatomic.

Inasmuch as this gas differs very markedly from argon in its spectrum, and in its behaviour at low temperatures, it must be regarded as a distinct elementary substance, and we therefore propose for it the name "metargon." It would appear to hold the position towards argon that nickel does to cobalt, having approximately the same atomic weight, yet different properties.

It must have been observed that krypton does not appear during the investigation of the higher-boiling fraction of argon. This is probably due to two causes. In the first place, in order to prepare it, the manipulation of a volume of air of no less than 60,000 times the volume of the impure sample which we obtained was required; and in the second place, while metargon is a solid at the temperature of boiling air, krypton is probably a liquid, and more volatile at that temperature. It may also be noted that the air from which krypton has been obtained had been filtered, and so freed from metargon. A full account of the spectra of those gases will be published in due course by Mr. E. C. C. Baly.

"Summary of the principal Results obtained in a Study of the Development of the Tuatara (*Sphenodon punctatum*)."
By ARTHUR DENDY, D.Sc., Professor of Biology in the Canterbury College, University of New Zealand. Communicated by Professor G. B. HOWES, F.R.S. Received June 15,—
Read June 16, 1898.

Thanks to the most generous and freely rendered services of Mr. P. Henaghan, Principal Keeper of the Lighthouse on Stephen's Island in Cook Straits, I have lately obtained a very perfect series of Tuatara embryos, ranging in age from just before the appearance of the blastopore to about the time of hatching. I have classified these embryos in sixteen stages, and propose shortly to publish a general

account of the development with numerous illustrations. As, however, it will still take some time to complete the drawings and manuscript, it appears desirable to publish at once a short summary of the most interesting results obtained. The general development, as already stated by Thomas, conforms closely to that of other reptiles, but the following features seem to deserve special mention :—

(1) The development occupies about thirteen months, the eggs being laid (on Stephen's Island) in November and hatched about midsummer of the year following. The last stages in the development, after about the first four months, occupy a much longer period than the earlier ones, so that, having reached an already very advanced stage, the development seems to be almost if not quite suspended during the winter months.

(2) The blastoderm spreads around the yolk at a very early date, and the embryo first appears as a cap-shaped mass of cells, the front end of which is elevated above the surrounding blastoderm as the head-fold, while the hinder and narrower end is formed by an undifferentiated mass of cells representing the primitive streak. The front part of the embryo is formed of epiblast and lower layer cells, and from the lowest of the latter the hypoblast is subsequently differentiated.

(3) In the primitive streak a distinct blastopore makes its appearance, which presently opens into the enteron below, forming a very distinct neurenteric canal which persists for some time.

(4) The notochord appears to be formed by a forward growth from the primitive streak in front of the blastopore, rather than by differentiation of hypoblast cells in the mid-dorsal line of the enteron.

(5) At a very early date the front end of the embryo sinks into the yolk, pushing the subjacent blastoderm before it in such a manner that the latter forms a kind of amnion closely investing the head and the thoracic portion of the body. This "amnion," though very thin, becomes differentiated into inner somatopleuric and outer splanchnopleuric portions, but, at any rate for a long time, without any mesoblast.

(6) At a comparatively late stage in development the anterior end of the embryo, together with the somatopleuric layer of the "amnion," is withdrawn from the splanchnopleuric layer (which belongs really to the yolk sac), and thus the embryo comes to lie entirely above the yolk sac.

(7) In the hinder part of the embryo the amnion is formed by uprising folds of somatopleure meeting and fusing above the embryo, probably accompanied by a down sinking of the embryo. This process is continued backwards for some distance behind the embryo, forming a narrow canal which communicates in part with the cavity of the true amnion, and opens behind on the surface of the blasto-

442 *Development of the Tuatara (Sphenodon punctatum).*

derm close to the sinus terminalis. The "posterior amniotic canal" thus formed is lined by epiblast, but it lies embedded in the mesoblast of the serous envelope which gradually splits off from the underlying yolk-sac around the embryo. The posterior amniotic canal arises at a very early date, and does not persist very long.

(8) The connection between the true amnion and serous envelope (false amnion) in the mid-dorsal line persists in part to a very late stage, but there is free communication between the two halves of the pleuroperitoneal space above the embryo.

(9) In connection with the vitelline circulation, very numerous absorbing vessels are developed which dip down far into the yolk, and large transparent globules of yolk, each surrounded by a layer of yolk "crystalloids," become arranged around these vessels like onions on strings. The yolk thus gradually assumes a very characteristic radially columnar structure.

(10) The parietal eye commences its development shortly after the appearance of the optic lobes. It arises by evagination of the roof of the brain in front of the prominence of the mid-brain, and is at first situated slightly to one side of the median line (the left side, so far as yet ascertained). It very soon becomes completely disconnected from its stalk as a closed, hollow vesicle, the wall of which is composed at first of a single layer of columnar cells. The outer (upper) part of the wall of the vesicle is thickened to form the lens and the inner (lower) part presently divides into two very distinct layers, and acquires a secondary, fibrous connection with the brain immediately in front of the stalk. It is a curious fact that while the parietal eye, after separating from its stalk, at first lies on the left side—the stalk itself is median.

(11) The posterior commissure arises just *in front* of the place where the stalk of the parietal eye connects with the brain and the stalk passes forwards above it. This fact seems to exclude the possibility of the stalk of the parietal eye representing the pineal gland, for, according to Balfour, the posterior commissure arises *behind* the pineal gland which is directed backwards.

(12) The pineal gland in *Sphenodon* appears to be represented by a mass of convoluted tubules lying in front of the stalk of the parietal eye.

(13) At a late stage of development (in embryos estimated at from four to eight months) the body and part of the head are marked with very distinct longitudinal stripes of white on a grey ground. This striping almost entirely disappears before hatching, being last retained on the under surface of the head. This observation is in close agreement with those of Eimer on the markings of mammals, &c.

(14) In embryos of the same age a patch of cornified epidermis

on the snout forms a very distinct "shell-breaker," which seems to be represented in the adult by the large median scale in front of the premaxillæ.

(15) At about the time of hatching there are on each of the premaxillæ three distinct, sharp, conical teeth, of which the outermost is the largest. These probably unite later on to form the large upper cutting teeth of the adult. There are also three similar teeth on each side in the front of the mandible, which probably unite to form the lower cutting teeth of the adult. (I have not found any vomerine teeth.)

Of the above results those regarding the amnion and the parietal eye seem to be the most interesting. I had discovered and drawn the posterior amniotic 'canal' long before I was aware of Mitsukuri's similar and previous discovery in Chelonians, and it appears to me that the observation is of especial interest in view of the supposed relationship between *Sphenodon* and the latter group, the probability of which is greatly strengthened by the striking similarity in the development of the foetal membranes.

The development of the parietal eye in *Sphenodon* certainly supports Béraneck's important conclusion that this organ (in *Lacerta* and *Anguis*) arises independently of the epiphysis, a conclusion which was also unknown to me until after I had come to suspect the same thing from observing the peculiar relation of the stalk of the parietal eye to the posterior commissure.

I may add that owing to the scarcity of biological literature in Christchurch the works of Mitsukuri and Béraneck above referred to are only known to me from the short abstracts in the 'Journal of the Royal Microscopical Society.'

"The Stomodæum, Mesenterial Filaments, and Endoderm of *Xenia*." By J. H. ASHWORTH, B.Sc., Demonstrator in Zoology, Owens College, Manchester. Communicated by Professor HICKSON, F.R.S. Received February 23,—Read June 16, 1898.

The specimen referred to in this note is a colony of the Alcyonaceous coral *Xenia* sp. ? from Celebes. The Xeniidæ are distinguished from all other Alcyonaria by their soft fleshy consistency and non-retractile polyps. The former character is due to the fact that their spicules are very minute rounded or oval discs (average measurements 0.015 mm. long, 0.01 mm. broad, 0.004 mm. thick), which have a horny basis impregnated with only a small quantity of calcium carbonate. The polyps have the usual Alcyonaceous structure, and

for the greater part of their length are bound together in bundles of about forty to sixty, each bundle forming one stem of the colony.

The polyp, or, more correctly, the free portion of the polyp, is 5 mm. to 7.5 mm. long, and bears eight tentacles, each 4 mm. to 5.4 mm. long.

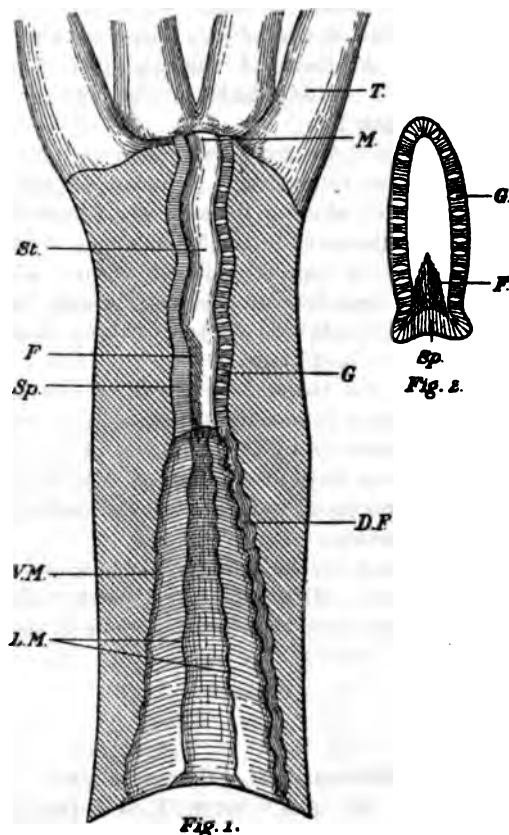


Fig. 1.

FIG. 1.—Semidiagrammatic view of one-half of a polyp which has been cut along the dorso-ventral line. Only the bases of the tentacles are shown. $\times 20$.

FIG. 2.—Transverse section through the lower third of the stomodæum (about the level of reference line F in fig. 1). $\times 80$. DF, dorsal mesenterial filament on the edge of the dorsal mesentery; F, flagella of siphonoglyphe; G, gland cells of stomodæum; LM, lateral mesentery (mesenterial filament absent); M, mouth; Sp, siphonoglyphe; St, stomodæum; T, tentacle; VM, edge of ventral mesentery (mesenterial filament absent).

The stomodæum of each polyp is 1.8 mm. to 2.2 mm. in length. It has a well marked ventral groove or siphonoglyphe, the cells of the lower third of which bear long flagella. Many of the cells of the

remainder of the stomodæum bear short cilia on their free surface, but among these are numerous "goblet cells" (G, figs. 1 and 2), which have not hitherto been noticed in the stomodæum of the Alcyonaria. These cells are swollen by some secretion to which they give rise; they generally appear to be empty, having discharged their secretion, which, in some cases, can be seen issuing from the cell into the cavity of the stomodæum. These secreting cells occur chiefly in the middle and lower portions of the stomodæum, and are most abundant on the lateral walls near the siphonoglyphe (see fig. 2). They do not occur among the cells which form the siphonoglyphe.

These "goblet cells" of the stomodæum are the only secreting cells connected with the digestive cavity which I have been able to find, as the six thick short ventral and lateral mesenterial filaments, which bear the gland cells in other Alcyonaria, are absent in all polyps of this *Xenia*. Only the dorsal mesenteries possess thickened edges forming the two mesenterial filaments which have a similar course and structure to those of *Alcyonium*. The free edge of the ventral and lateral mesenteries is only very slightly thickened, and the cells which cover this edge differ in no way from those which cover the remaining portions of the mesentery.

The two new points in the anatomy of this species of *Xenia* are the presence of gland cells in the stomodæum and the absence of the six ventral and lateral mesenterial filaments usually present in the polyps of the Alcyonacea. Wilson* (in *Kophobelemnion*) and Hickson† (in *Alcyonium*) have shown that these mesenterial filaments bear the cells which produce the digestive secretion. I would suggest that the absence of these filaments in this *Xenia* is correlated with the presence of gland cells in the stomodæum, and that these cells, judging from their appearance and position, probably perform some digestive function.

The siphonozooids which occur in Pennatulids and some other Alcyonaria are the only recorded examples of polyps in which the ventral and lateral mesenterial filaments are absent. According to Wilson these siphonozooids derive their food-supply from the autozooids or feeding-polyps, and, therefore, do not require cells to produce a digestive secretion.

In this species of *Xenia*, then, the secretion in connection with the digestive cavity is formed, not by endoderm cells, but by cells which are derived from the ectoderm, as, from a study of the buds, I have found that the stomodæum is ectodermic in origin in this as in other Alcyonaria.

The endoderm cells which line the coelentera and the cavities of the tentacles have a similar structure throughout the colony. They are

* 'Mitt. Zool. Stat. Neapel,' vol. 5, 1884.

† 'Quart. Journ. Micr. Science,' vol. 37, 1895.

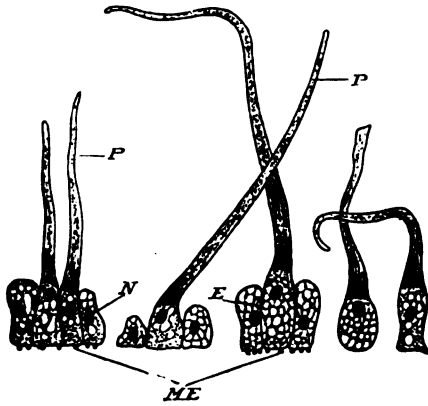


FIG. 3.—Endoderm cells from coelentera of polyps, showing the ordinary endoderm cells (E) and the cells bearing pseudopodia (P). $\times 400$. E, endoderm cell with vacuolated protoplasm; ME, muscle process of endoderm cell cut transversely; N, nucleus; P, pseudopodia of endoderm cells.

cubical or columnar, and contain many small vacuoles which give the protoplasm a reticulate appearance (E, fig. 3). Among the ordinary endoderm cells there are numerous cells, the inner or free end of which is produced into a long pseudopodium (P) which is from four to eight times as long as the basal portion of the cell. This pseudopodium may be slender or moderately stout, and may attain a length of over 0.12 mm. The basal part of the cell from which the pseudopodium arises has the reticulate protoplasm of an ordinary endoderm cell, and the nucleus of the cell is situated in this portion. The pseudopodium is not vacuolated; its protoplasm exhibits a homogeneous or very finely granular structure. The pseudopodia usually taper towards their free end, but in a few instances this end is slightly broadened and flattened. In one case the pseudopodium, which is a very large one, bears a short branch near the middle of its length. The pseudopodia appear to be flexible, as in several cases they are curved so that their tips approach the basal portion of the cells. These curious pseudopodia-bearing cells are very numerous, and are found in all parts of the endoderm lining the coelentera and the cavities of the tentacles.

"On Surfusion in Metals and Alloys." By W.C. ROBERTS-AUSTEN, C.B., D.C.L., F.R.S. Received May 20,—Read May 26, 1898.

(PLATES 8, 9.)

Introduction.

The fact that metals and alloys may be maintained in a fluid state at temperatures which are many degrees below their true freezing points, has been but little studied. As regards salts the question of surfusion has recently received much attention. Ostwald* has shown, as the result of an investigation of great interest, that a very minute quantity of a solid will cause a mass of the same substance to pass from the surfused to the solid state. His work, moreover, has led him to distinguish between the *meta-stable*, or ordinary condition in which surfusion takes place, and the *labile* condition which occurs at temperatures much below the melting point. Ostwald's paper, and one by M. Brillouin,† on the theory of complete and pasty fusion lead me to offer the Royal Society the results of some experiments of my own on the surfusion of metals.

Historical.

Metals do not appear to have been studied from the point of view of surfusion until the year 1880, when some excellent experiments on the surfusion of gold were made by the late Dr. A. D. van Riemsdijk,‡ by whose early death, which occurred last year, Holland has lost a skilful physicist. He pointed out that:—

"Faraday, in his memoir on regelation, published in 1858, stated that acetic acid, sulphur, phosphorus, many metals and many solutions, may be cooled below the congealing temperature prior to solidification of the first portions."§ On the other hand, in their treatises on physics, Danguin|| and Jamin¶ mention tin as the only metal which is capable of remaining liquid at a temperature 2·5° below the true melting point of the metal, which is 228° C.

Van Riemsdijk's contribution to the subject of surfusion of metals consisted in showing that the well-known phenomenon of *éclair*, the brilliant flash of light which often attends the solidification of the metal in the ordinary assay of gold, is really due to surfusion. He

* 'Zeit. für Physikal. Chem.,' 1897, vol. 22, p. 3.

† 'Ann. de Chim. et de Phys.,' 1898, vol. 18, p. 264.

‡ 'Ann. de Chim. et de Phys.,' 1880, vol. 20, p. 66.

§ 'Experimental Researches in Chemistry and Physics,' p. 379.

|| Vol. 1, 1855, p. 892.

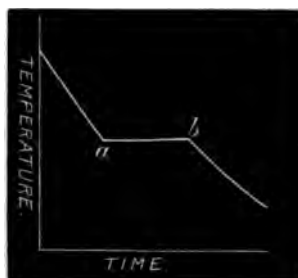
¶ Vol. 1, 1859, p. 105.

also pointed out that surfusion could be either stimulated or hindered by suitably modifying the conditions, but he made no attempt at thermal measurements. It was not until ten years after the publication of van Riemsdijk's work that the recording pyrometer, which I devised and submitted to the Royal Society in 1891,* enabled such measurements to be readily effected.

It consists of a camera, enclosing a dead-beat galvanometer, to which the free ends of a thermo-junction are attached. The thermo-junction is suitably protected, and, as it only consists of two wires, twisted together and covered with a fine clay tube, it can be placed in the cooling mass of molten metal or alloy, and the cooling curve of the mass may be traced by a spot of light from the galvanometer which falls on a moving sensitised plate. A ready method for studying all the phenomena of the solidification of metals and alloys is thus afforded.

The freezing point of a metal, or the initial freezing point of an alloy, for a fluid mass of two metals may possess many points of solidification, is represented by one or other of three typical curves. These are shown in the accompanying figures which indicate the

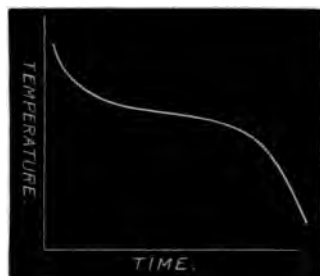
FIG. 1.



nature of the curves, traced by the recording pyrometer. Fig. 1 shows the freezing point curve of a pure metal, the horizontal portion, *ab*, indicating the actual solidification of the mass, the sharpness of the angles at *a* and *b* attesting the purity of the metal. The initial freezing point of most alloys would resemble fig. 1 in having the corner *a* sharp, while the point *b* is generally rounded off. If the alloyed metals form an isomorphous mixture, neither angle is sharp, and in many cases there is no true freezing point, the curve being of the form shown in fig. 2. This represents the freezing of the gold-silver alloy containing 28 per cent. of gold in which the fluid mass, as a whole, passes through a long pasty range

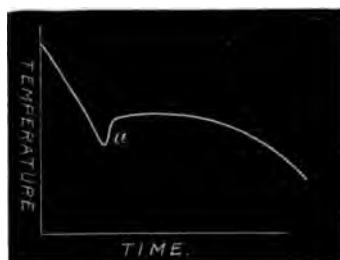
* 'Proc. Roy. Soc.,' 1891, vol. 49, p. 347.

FIG. 2.



before solidifying. The third type of curve, which may be a modification of the other two types, indicates the occurrence of surfusion, and, as a case in point, pure gold has been taken, the bend at *a*, fig. 3, showing the amount of surfusion which was observed. I have detected pronounced cases of surfusion not only in gold but in copper, bismuth, antimony, lead, and tin. Surfusions, moreover, is not confined to pure metals, and I showed in 1893* that the eutectic alloy in the bismuth-copper series presents a marked case of surfusion.

FIG. 3.



In order to study surfusion, it is necessary to make the galvanometer, to which the thermo-junction is attached, very sensitive, and, by suitable adjustment, it is easy to catch on the sensitised plate any desired portion of the range of the spot of light. It is, however, preferable to balance by a potentiometer the current which results from the heating of the thermo-junction, and in this way to prevent the mirror from swinging through a long range. The sensitiveness of the instrument is but slightly diminished by the introduction of the potentiometer. As the thermo-junction cools down, the spot of light from the galvanometer is simply made to traverse a short distance many times, instead of a long range once. The sensitised

* "Second Report, Alloys Research Committee," 'Mech. Eng.,' 1893, Plate 22.

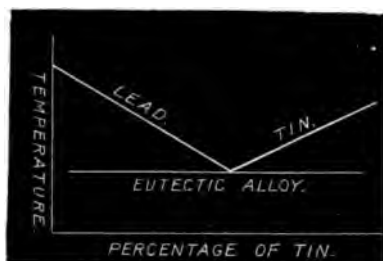
plate need only be exposed to the action of the spot of light at the critical moment, when solidification or surfusion is known to be imminent. A paper by my assistant, Mr. Stansfield, illustrating the use of this method, will shortly be published by the Physical Society.

A curve, traced by the aid of either of the sensitive methods which have just been described, if it represents the surfusion of a metal or alloy does not merely show a slight depression as in the case of pure gold shown at *a*, fig. 3: the slight depression becomes a deep dip, (Plate 8, fig. 1, which represents the surfusion of gold). In this case three curves were taken on one plate, the line *ab* represents the heating and melting of gold, the horizontal portion marking the actual melting of the metal. The lines *cde* and *fgh*, on the other hand, represent successive coolings and solidifications of the metal, surfusion occurring in both cases, the horizontal portions at *e* and *h* representing the beginning of solidification of the gold in each case. It is noteworthy that these successive points of solidification differ by less than half a degree, the melting point of the metal (the line *ab*) occupying a median position between them. During the surfusion, the temperature of the metal fell about 2° below its true freezing point. Figs. 2 and 3, Plate 8, both represent the surfusion of gold; fig. 4, Plate 8, that of copper,* while fig. 5, Plate 8, represents the surfusion of an alloy of antimony with 25 per cent. of copper which freezes at 520° . It was selected as representing the highest temperature of surfusion which I have as yet examined in the case of an alloy. Such a surfusion curve, in the case of an alloy, may do much more than reveal the sudden release of the latent heat of the fluid mass. The curve may present a complicated record of modifications in molecular grouping occurring in the brief space of time actually occupied by surfusion. This is shown by the points *d*, *e*, in fig. 6, Plate 9, which represents the surfusion of tin alloyed with 36.5 per cent. of lead. All the curves on Plate 9 serve as bonds connecting the behaviour of alloys with that of freezing solutions of salts.

The explanation of the existence of these points, *d*, *e*, is simple. The freezing point curve of the lead-tin series is a very ordinary type, and is shown in the accompanying diagram (fig. 4), and, as regards the portion where the lines meet, full details are given on a larger scale in fig. 5. It consists of two branches united at the point where they join a horizontal line which represents the freezing of the

* Figs. 2, 3, 4 on Plate 8 were taken with an insensitive galvanometer and have been enlarged by photography from the original plates. Fig. 5, Plate 8, and all the curves on Plate 9 are given exactly as they were taken on the plate of the recording instrument. The co-ordinates, as in the case of the figures 1 to 4, are time and temperature.

FIG. 4.



eutectic alloy of the lead-tin series. This alloy freezes at a constant temperature (183°). The eutectic constitutes a fluid residual "liquor" which is left after the excess of either lead or tin has fallen out as the mass cools down. When, however, the tin is present in slight excess of the amount required to constitute the eutectic, the whole mass of the alloy may remain fluid at temperatures below that at which the eutectic would naturally freeze. It may even cool to a temperature at which it can no longer maintain all its *lead* in solution, and some lead will, therefore, fall out while surfusion is actually taking place. This explains the existence of the point *d*, which marks an arrest in the fall of temperature, in Plate 9, fig. 6 (on the line *ab*, representing the *fall* of temperature during surfusion). Conversely, when the surfusion is ended, the latent heat is released, and the line *bec* represents the *rise* of temperature due to the release of latent heat. There should also be on this line an indication of a retardation or arrest in the rise of temperature, because the lead which fell out of solution (at the point *d*) has to be re-melted. This point of arrest will be found at *e*.

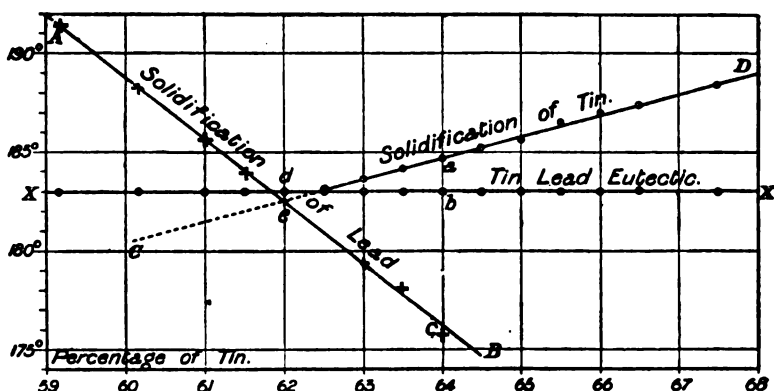
In the same way, in fig. 7, Plate 9, which represents a fine case of surfusion in the lead-tin alloy containing 64 per cent. of tin, there is also a point at *f*, on the line *gh*, at which point the lead fell out of solution during the surfusion. Fig. 8, Plate 9, represents the surfusion of a lead-tin alloy containing 68 per cent. of tin. In this case the tin is in large excess, and freezes first. In becoming solid, the tin would have been represented by a horizontal line somewhere about the point *s*, on the line *ij*, had not surfusion occurred, and been terminated by the solidification of tin at *k*. This is followed by the solidification of the eutectic mother liquor represented by the line *lm*. This eutectic does not surfuse because the tin (the metal in this alloy which is prone to surfuse) is already present in the crystalline form.

In fig. 9, Plate 9, a similar case is represented, but a small, though distinct, surfusion at the point *n* has been recorded. This surfusion

at n probably indicates that the lead in the eutectic may surfuse slightly even though the crystallisation of tin has begun.

By taking a series of lead-tin alloys which do not contain more than 2 or 3 per cent. of tin, in addition to the amount required to constitute the eutectic (62 per cent. of tin), the temperatures at which the lead begins to crystallise below 183° , can be measured. The results are shown in the diagram, fig. 5. In it the co-ordinates are tem-

FIG. 5.—Freezing-point curves of lead-tin alloys.



peratures and percentages of tin. Each alloy examined has, it will be seen, at least two freezing points, and some appear to have three, but in all the alloys one of the freezing points (the eutectic) is at the constant temperature 183° . In the eutectic alloy (62 per cent. of tin), theoretically these points, d , e , should coincide. In fig. 5 they are not quite coincident, and this is due to the fact that the lines AB , CD , drawn through the observed freezing points, are of necessity somewhat lower than the ideal solubility curves. The freezing points which lie below the horizontal or eutectic line were recorded while surfusion occurred. For instance, take the alloy containing 64 per cent. of tin, its initial freezing point would be at a (185°), that of its eutectic at b (183°), while the third freezing point, c (176°), is due, as has already been explained, to the falling out of lead while the fluid mass was in the surfused condition. In order to observe the uppermost of the three points (185°), it is necessary to stir the mass to prevent the possibility of surfusion. On the other hand, a record of the lowest freezing point can only be obtained in a fluid mass which is perfectly tranquil. When lines are drawn through all the points recorded it will be seen that the line AB , representing the solidification of lead, extends far below the horizontal line XX . It should be remembered that if the fluid mass

SURFUSION.

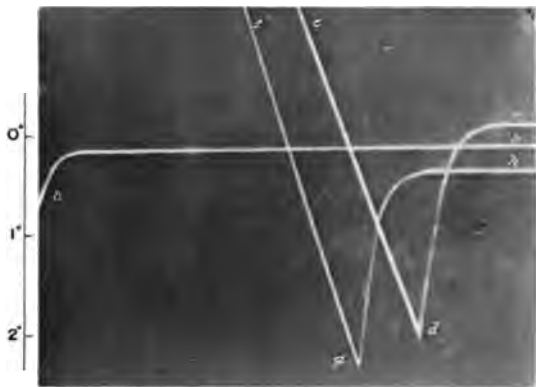


Fig. 1. Gold.

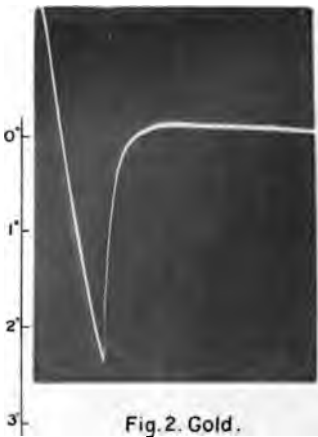


Fig. 2. Gold.

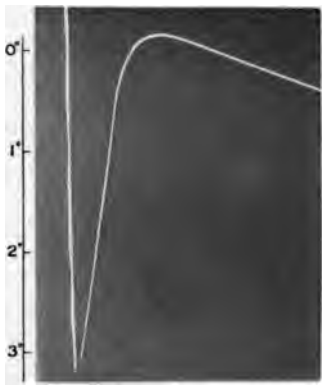


Fig. 3. Gold.

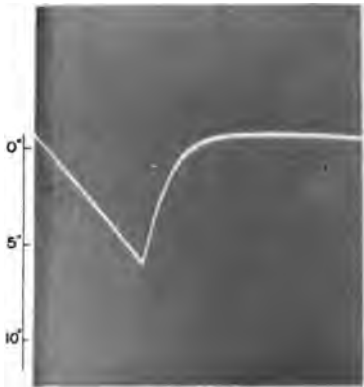


Fig. 4. Copper.

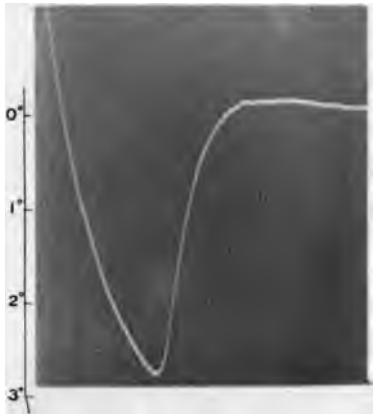


Fig. 5. Antimony
with 25% Copper.

SURFUSION.

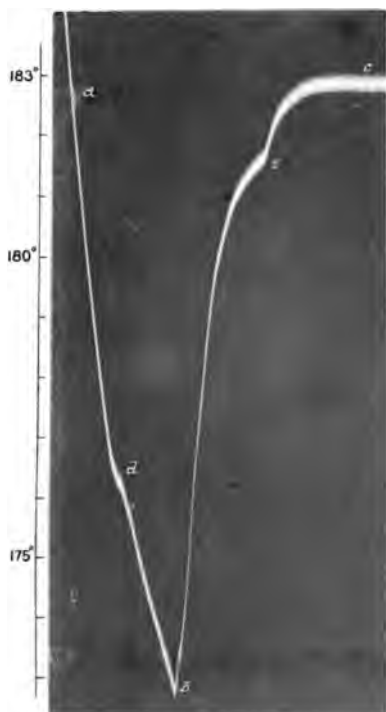


Fig. 6. 64 Tin, 36 Lead.

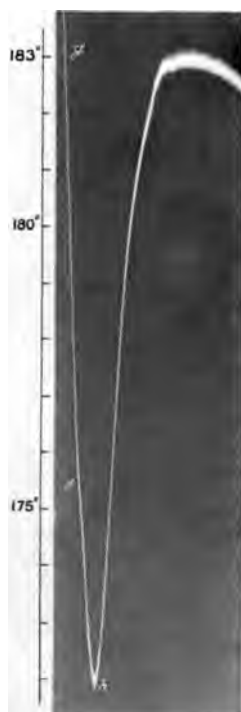


Fig. 7. 63.5 Tin, 36.5 Lead.

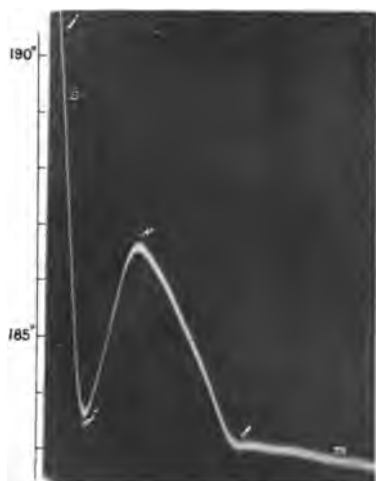


Fig. 8. 68 Tin, 32 Lead.

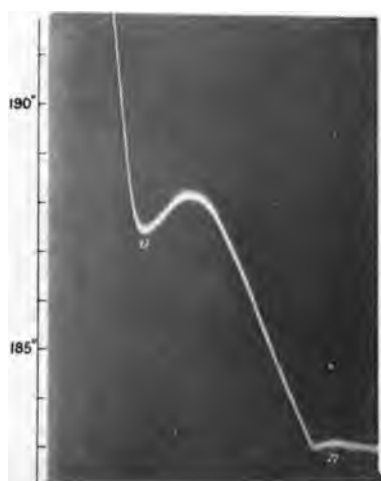


Fig. 9. 68.5 Tin, 31.5 Lead.

is stirred, the first point to be recorded will be the upper or tin point (185°), followed by the eutectic point (183°). If the alloy surfuses, the first point recorded will be the lowest point (176°), followed in turn by the eutectic. In general, therefore, only two freezing points can be obtained in a single record.

In the case of salts the crossing of the curves of solubility has already been observed by H. le Chatelier and by Dahms, but in the case of alloys, experimental evidence has hitherto been wanting. The silver-copper series presents many analogies to the lead-tin series. Heycock and Neville*, in their excellent work on the complete freezing point curves of many series of alloys, have calculated what the ideal freezing point curve of the silver-copper series would be, but the present paper affords, I believe, the first experimental evidence as to the identity of the behaviour of saline solutions and metallic alloys as regards selective surfusion.

Prolonged experience in these methods of manipulation may prove that it is possible to effect the separation of a particular metal or definite groups of metals by dropping in (during the surfusion of the fluid mass) a fragment of the same metal or of the particular group of the associated metals it is intended to separate. It is well known that the introduction of a fragment of the same metal or of an isomorphous metal or alloy will determine its solidification. Such a method may readily be employed in studying the surfusion of salts. In the case of metals, so far as my own experiments go, the surfused state is singularly unstable, for it may be disturbed even by very slight tremors. It remains to be seen whether it is possible to arrange the experiments in such a way as to maintain metals and alloys for an indefinite time in Ostwald's meta-stable condition which would need the presence of a particle of solid matter to induce the solidification of the mass.

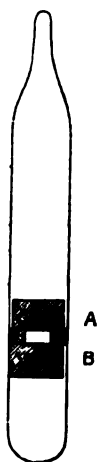
Ostwald applies to the change from liquid to solid the equation which represents the gas-liquid change. In the former case there are, however, the three phases, solid, liquid and gas, present, and a complete expression of the change must take account of all three. Thus during surfusion the gas phase is in equilibrium with the liquid phase, but when solidification has begun the gas phase must also be in equilibrium with the solid phase. It is possible that the solidification of the mass may be started by crystals deposited directly from its vapour so that solidification of a surfused metal may be started by crystals from its own metallic atmosphere. The amount of gas evolved by a solid metal is, of course, very small. Evidence of the vaporization of metals at very moderate temperatures is not wanting. Demarçay† showed in 1882 that *in vacuo* metals evaporate

* 'Phil. Trans.,' A, vol. 189 (1897), p. 25.

† 'Comptes Rendus,' 1882, vol. 95, p. 183.

at much lower temperatures than they do at ordinary atmospheric pressure. Pellat* pointed out that the proximity of the surface of a metal to that of another metal in air, changes its electrical condition, and he attributed this to vaporization of metals, showing that even iron exerted an influence at a distance. Colson† showed that a photographic plate was affected by the vapour of certain metals, notably by zinc, cadmium, and magnesium even through porous septa. Dr. Russell‡ in some recent and very interesting

FIG. 6.



experiments, was led to the conclusion that even so infusible a metal as cobalt will by vaporization, affect a photographic plate. In January, 1897, before hearing of Dr. Russell's experiments, I began some experiments with a view to ascertain whether metals, vaporized *in vacuo* near the ordinary temperature, will actually unite to form alloys. The arrangement is shown in fig. 6. A and B are two discs of metal with polished surfaces separated by a ring of glass, the whole being enclosed in a vacuous tube which could be heated in a water bath. I found that when cadmium and silver were opposed for eight days at a temperature of 50° an appreciable deposit of a tinted cadmium-silver alloy formed on the surface of the silver. Cadmium must, therefore, have passed across the interval between the discs A and B.

The results given in the present paper, reveal additional points of similarity between the behaviour of alloys and that of ordinary saline solutions. I trust, therefore, that it may be useful as a continuation of my investigation on the "Diffusion of Metals," which formed the subject of the Bakerian Lecture of 1896.

"Experimental Investigations on the Oscillations of Balances."

By D. MENDELEEFF, For. Mem. R.S. Received June 9,—
Read June 9, 1898.

In the year 1893 the Central Chamber of Weights and Measures (Glavnaya Palata Mer y Vesov) was created in St. Petersburg to act as a Central Institution of the Empire for the verification of all kinds of standard measures. Having been appointed Director of the above-mentioned Institution, I was first of all occupied in

* 'Comptes Rendus,' 1882, vol. 94, p. 1247; 1896, vol. 123, p. 104; and 1898, vol. 126, p. 1338.

† *Ibid.*, 1896, vol. 123, p. 49.

‡ 'Roy. Soc. Proc.,' 1897, vol. 61, p. 424, and *ibid.*, vol. 63, p. 102, Bakerian Lecture, 1898.

making arrangements for accurate weighings, whereby some improvements have been introduced in the usual methods; a detailed description will be found in the official report on the renewal of Russian standards of measure and weight.

The results of the work show that with the balances of our Institution we are able to find the difference between two platinum-iridium kilogram weights by one single weighing with an accuracy of ± 0.02 milligram, and by a system of weighings to ± 0.002 milligram with a probable error of a few ten-thousandths of a milligram.

We have a number of such balances, but those mostly used were constructed by the well-known balance makers of Vienna, Ruprecht and Nemetz; some important improvements have been made upon these balances, especially in the direction of minimising the influence of the observer.

Having obtained such results in accurate weighings, I have used them, not only in the comparison of standards of weight, but also in an experimental research on the oscillations of a balance, hoping to collect some material, not only for an investigation of balances of different systems and constructions in general, and of the friction of the knife-edges, but especially to find the action of gravity and moment of inertia on such a pendulum as is represented by an accurate balance.

If we consider the time of one oscillation (from 30 to 60 seconds), we find that our balances correspond to synchronous mathematical pendulums of a length equal to 1000—3000 metres. The investigations in this direction are not yet completed, but one part of the results obtained is now in print, and I would like to communicate an abstract to the Fellows of the Royal Society.

Before going into the matter, I must explain that the many hundreds of observations of the times of oscillation and the changes in the scale-reading have been made by my friends and assistants, especially F. P. Zavadsky, V. D. Sapogenikoff, and also by Messrs. Dobrokhotoff, K. Egoroff, Miller, and Misses Ozarovskaya and Endymionova. Their active co-operation has greatly contributed to the success of the experiments, and I am much indebted to them for verifying the numerous calculations which this research involves.

In the investigations mentioned, the following data were observed:—

1. The readings of the scale, l_n , were observed through a telescope after reflection from a mirror attached to the beam of the balance, l_1 is the first reading. One division of the scale corresponds in the different balances to an angle of from 0.5 to 4 minutes. The readings were taken by estimation to 0.05 division, which agrees nearly with the probable error in l .

From every set of four to five readings the position of equilibrium L_n was deduced, and the amplitudes r_n , by taking the differences between l_n and L_n . The difference between r_n and r_{n+1} is called in what follows the decrement D_n .

2. The time of the passage through the position of equilibrium, T_n , was determined partly by the use of a chronographic watch, partly by Marey's cylindrical chronograph, reductions having been made to true astronomical mean time by comparisons with our standard clock (Hohwü 31) controlled by signals from the Pulkova Astronomical Observatory.

From the observed T_n was deduced the mean duration (in seconds of mean time) of one oscillation, i.e., from l_n to l_{n+1} .

3. k , the number of milligrams corresponding to one division of the scale.

4. e , the weight in milligrams of a litre of air inside the balance case, according to readings of the thermometer ($\pm 0.003^\circ$), barometer and Assman's psychrometer.

5. p , the weight of the load on each pan expressed in grams.

6. P , the weight of the whole moving mass of the balance and the load in grams.

7. v , the volume of the load in millilitres. The same has an influence on t_n and D_n , and we have examined the change of D_n and t_n .

I. *The Variation of D and t depending on the value of one oscillation,*
 $R_n = l_n - l_{n+1}$, or amplitude, $r_n = (L_n - l_n)(-1)^n$.*

The duration of one oscillation, all other conditions being constant in all six examined balances without exception *decreases with decreasing oscillations or amplitudes*. These variations are not only many times greater than the errors of the readings, but many hundred times surpass the corrections of the time of one oscillation, as deduced from the usual formula for the reduction of the oscillations of a pendulum to infinitely small amplitudes. The decrease of the time of one oscillation in the most simple manner can be expressed in first approximation by the formula

$$t_n = t_0 \beta^{-n} \dots \dots \dots (1).$$

Therefore

$$T_n = Q - U \beta^{-n},$$

where

$$U = \frac{t_0}{\log. \text{ nat. } \beta^*}$$

and

$$Q = T_0 - U.$$

* r_n is very nearly equal to $\frac{1}{2}R$, but I prefer to give D and t in relation to r_n , because by this method the small errors in L_n disappear.

More clearly visible is the *decrease of D with decreasing amplitude* τ_n , and it is sufficient to observe four readings. The experimental law of the decrease of D by first approximation can be represented by the formula

$$D_n = d + \alpha \tau_n,$$

where d is the limit of the decrement in the case of infinitely small amplitudes, and α a constant coefficient, which in all examined balances varied between 0.0010 and 0.0002. Observations relating to t_n and D_n have been made in a very great number, and all of them confirm the above given result.*

Therefore
$$\tau_n = \frac{r_0}{\frac{\alpha r_0 \cdot d^n - 1}{d - 1} + d^n},$$

or
$$\frac{1}{\tau_n} = K d^n - N,$$

where
$$N = \frac{\alpha}{d - 1},$$

and
$$K = N + \frac{1}{r_0}.$$

Similar facts about t and D have been shortly mentioned before,† but little notice of them has been taken. But in our very numerous observations these facts stand out so clearly and beyond doubt, that also in the ordinary simple pendulums we must suppose the existence of similar deviations which only by their smallness have escaped the attention of the observers. I have commenced the investigation of a simple pendulum in this direction.

As D and t are varying with the amplitude τ_n , in what follows D and t will be given for the case when $r = 15$ divisions, using the signs D_{15} and t_{15} .

II. Variation of D_{15} and t_{15} with varying load.

The time t_{15} in all balances decreases with decreasing load and k .

(a) For one kind of balances very quickly, as, for instance, Rapprecht's balance:—

	1.	2.	3.	4.
p	0 grms.	105 grms.	430 grms.	563 grms.
P	1234 „	1440 „	2099 „	3358 „
D_{15}	1.0136	1.0180	1.0217	1.0244
t_{15}	27.3 secs.	28.5 secs.	38.3 secs.	48.2 secs.

* For details see the above mentioned Official Report.

† Cf. O. E. Meyer, 1871, Mercadier, 1876, T. Thiessen, 1886, and others.

(b) For other balances considerably slower, as for the balance of Nemetz :—

P	1147 grms.	1359 grms.	2213 grms.	3442 grms.
k	0.0192 mgrs.	0.0234 mgrs.	0.0481 mgrs.	0.0670 mgrs.
D ₁₅	1.0321	1.0392	1.0337	1.0313
t ₁₅	30.5 secs.	32.8 secs.	33.3 secs.	33.7 secs.

The decrement D₁₅ in some balances with increasing load (a) increases as with the above-mentioned balance of Ruprecht, or (b) decreases as with a balance of Collot designed to carry a load of 40 kilograms on each pan :—

$\frac{p \text{ grm.}}{409.5 \text{ grm.}} = 0.$	1.	2.	4.	8.
P .. 29,879 grm.	38,069 grm.	46,259 grm.	62,640 grm.	95.402 grm.
D ₁₅ .. 1.0459	1.0316	1.0259	1.0172	1.0103
t ₁₅ .. 55.9 secs.	53.0 secs.	49.3 secs.	43.5 secs.	35.9 secs.

In this case the variation of D in relation to *p* corresponds in first approximation to a hyperbola with an asymptote = 1 or (c), a combination of the preceding (a) and (b).

III. Variation of D₁₅ and t₁₅ with the Variation of the Sensibility of a Balance 1/*k*.

If *k* increases, all other conditions being the same, then D₁₅ and t₁₅ decrease as with the balance of Nemetz. (P = 2213 grams.)

k	0.034 mgrms.	0.055 mgrms.
t ₁₅	37.3 secs.	30.6 secs.
D ₁₅	1.0363	1.0286

IV. Variation of the Time of one Oscillation depending upon the Friction of the Knife-edges (Prisms) against the three Planes.

When the pans and the movable parts used for suspending them are removed and the balance beam alone is oscillating, there is friction only at the middle knife-edge, and the time of one oscillation is shortened, as with the above mentioned balances of Collot and Ruprecht; for the latter we have found—

	With pans but without any load.	Beam only.
P	1231 grms.	678 grms.
k	0.0310 mgrms.	0.0290 mgrms.
t ₁₅	27.3 secs.	15.0 secs.
D ₁₅	1.013	1.007

If in the balance of Nemetz the pans are suspended from the beam,

and we put in the place of the middle agate plane (the knife-edges being of flint) planes of the same size but of different material, we find a change in the time of the oscillation; from thirty-two oscillations in each case we have obtained the following mean results ($P = 2178$ grams) :—

Material of the middle plane.	Time of one oscillation, t_{18} .	D_{18} .	k .
Hard steel	33·3 secs.	1·034	0·043
Agate	32·7 „	1·031	0·044
Copper	25·4 „	1·065	0·071
Horn	18·2 „	1·115	0·093

That is to say, the time of one oscillation in the last case is one-half of that when an agate plane is used, although the load and the position of the centre of gravity were the same in both cases.

Therefore the experiment shows a great dependence of the time of one oscillation decrement and of the sensibility from the friction of the knife-edges, although usually it is assumed that t and k do not directly depend upon the friction. Further experiments are going on, especially as regards the changes found in the value of the decrement.*

V. *Variation of t and D depending upon the Dimensions of the Horizontal Section of the Load.*

If we put on the pans of the Ruprecht's balance two platinum-iridium pounds (Russian) (cylinders of which the height and diameter are equal and the weight 409·5 grams), we find $k = 0·033$ milligram, t about 44·4 secs., and D about 1·0175; replacing these weights by two plates of 914 square cm. section (of the same weight 409·5 grams), we find t about 39·7 secs., and D about 1·0240.

VI. *Variation of t and D with the Variation of the Volume of the Load.*

VII. *Influence of the Variation of the Density of the Air e , and*

VIII. *The Inner Friction of the Gas in which the Oscillations are going on.*

The experiments will be continued and extended, and I hope to obtain results which will throw more light on the little explored regions of the oscillation of a balance considered as a physical pendulum of peculiar properties.

Some results will probably be applicable to the ordinary physical pendulum.

But, for the moment, I would not like to make any theoretical generalisations.

* *Vide* Bessel and F. Bailey, 1832, "Pendulum Experiments."

“On the Determination of the Magnetic Susceptibility of Rock
By A. W. RÜCKER, Sec. R.S., and W. H. WHITE, A.R.C.
Received July 6, 1898, and published during the vacatic

In the Report of the Magnetic Survey of the United Kingdom the question as to how far the local and regional magnetic disturbances of the earth's field may be ascribed to the influence of masses of magnetic rocks was discussed by Professors Rücker and Thorpe.†

In a previous paper‡ a method for the determination of the magnetic susceptibilities of rocks was given, together with values for the specimens then examined. It was considered desirable to add to this relatively small number of results so as to obtain a broader basis for the development of the theory. Accordingly Sir Archibald Geikie, F.R.S., Director-General of the Geological Survey, has been good enough to supply us with a typical series of specimens of the basic rocks of the country, and, after a complete overhauling of the method originally adopted, we have measured the susceptibility of sixty-eight of these new specimens of forty-five different rocks. The earlier stages of the work were carried out by Mr. F. Fisher, A.R.C. who would, no doubt, have taken part in the whole had he not left the college for an appointment elsewhere.

The principle of this method of determining the susceptibility of small pieces of feebly magnetic materials is to compare the susceptibility of the fragment with that of the displaced volume of liquid of known susceptibility in which it is immersed.

In more detail, the method is as follows:—

(a) To measure the susceptibility of the magnetic liquids a tube containing the liquid is introduced into one of two equal solenoids the effects of which on a small magnetometer destroy each other. In these experiments a field of seven absolute units was employed. The ratio of the deflection caused by the tube of liquid to that produced by a measuring solenoid traversed by a small current, multiplied by the computed constant of the instrument, gives the absolute susceptibility of the liquid.

The apparatus was figured and described on pp. 506—509, ‘Roy. Soc. Proc.,’ vol. 48, 1890.

(b) The differences of susceptibility of rocks and liquids are measured on the calibrated sonometer scale of a Hughes' induction balance. Two tubes, filled with a liquid to the sufficient depth of 6 cm., are balanced, and the distance through which the compensa-

* ‘Phil. Trans.,’ vol. 188, 1896.

† *Ide* pp. 637—647 and map 14.

‡ ‘Roy. Soc. Proc.,’ vol. 48, 1890, p. 505.

has to be moved to produce silence after the introduction of a small piece of a rock into one of the tubes is noted. This is repeated with liquids of different strengths. Then if a , b are the differences observed between the rock of susceptibility x and two liquids of susceptibility k_1 , k_2 respectively, we have

$$\frac{a}{b} = \frac{K_1 - x}{K_2 - x} \quad \therefore x = \frac{bK_1 - aK_2}{b - a}$$

due regard being given to the signs of the differences.

The liquid employed was pure glycerine in which finely divided magnetic oxide of iron was suspended, for no solutions of metallic salts possess a susceptibility sufficiently high for our purpose. These liquids were very carefully prepared, and in the light of the experience now gained it is desirable to add to the statements made in the earlier paper on this point. Natural magnetite cannot be ground fine enough to remain long in suspension, though experiments in which a coarsely powdered mixture of magnetite and manganese dioxide (minerals of about the same specific gravity) was shaken in water and allowed to settle rapidly into a solid mass were fairly successful.

Finally, the magnetic oxide was prepared artificially by adding ammonia to a boiling solution of sulphate of iron of which one-half had been oxidised by nitric acid. The black precipitate, dehydrated by alcohol and dried at 100°, gives a friable dark brown mass about one and a half times as magnetic as a good specimen of the powdered mineral.

By grinding with glycerine between glass plates the oxide is reduced to minute yellow scales. Glycerine mixtures made with this substance give no indications of settling for forty-eight hours.

Numerous experiments with various suspensions of natural magnetite called attention to the fact that in a magnetic field the particles rotated slowly to set their axes along the lines of force, thus giving a fictitious susceptibility largely dependent on the permanent magnetisation of the grains of magnetite. With the artificial oxide this effect is extremely small, but, to avoid it altogether, we prepared gelatinised glycerine mixtures which at ordinary temperatures become solid. In this state we repeatedly determined, firstly, their absolute susceptibility with the magnetometer apparatus, and, secondly, the value in sonometer scale divisions of each balance tube filled with the jelly. With these particular tubes one division = susceptibility 0.0001075. The balance tubes being thus standardised, the absolute magnetic value of a liquid is determined by the induction balance under exactly the same conditions that hold during the tests of rocks.

Sensitiveness and Accuracy.

The errors arising from various sources have been carefully considered. They naturally fall into two groups—

First. Errors in the determination of the primary standard of susceptibility, and affecting all results equally.

Second. Errors affecting individual specimens.

1. Under the first heading come the probable errors in the magnetometer constant and readings.

In the value of the constant there is an estimated probable error of ± 1 per cent. Combining with this several smaller errors due to temperature, variation of resistances, imperfect adjustment, external magnetic disturbances, &c., we conclude that the susceptibility of each standard jelly is subject to a probable error ± 2 per cent. The two jellies actually gave factors 0.000109 and 0.000106 (the error of constant being the same in each case). *Presumably the mean factor 0.0001075 is within ± 2 per cent. of the truth.*

2. Errors in the values of individual rocks arise from the limited sensitiveness of induction balance, telephone, and ear. The values of the magnetic liquids are not appreciably affected by this, but it becomes important in the case of rock specimens of small size and feeble susceptibility.

In the earlier paper already referred to it was stated 0.00013 was the lower limit to the range of measurement by the methods adopted and experiments made since with magnetic and non-magnetic substances indicate about the same degree of accuracy. For moderate susceptibilities, however, the accuracy now attained has been greater than in the earlier work, and the errors cannot in most cases be much greater than 1 per cent. The difference between two specimens taken from the same rock is of course greater than this, as the composition of the rocks is not uniform.

It is not necessary to give a detailed example of an experiment. Suffice it to say that in a particular case the susceptibility of the rock (No. 2) was determined by comparison (1) with liquids H and D of susceptibilities 0.00074 and 0.00357, and (2) with liquids H and C of susceptibilities 0.00074 and 0.00250. The results obtained were 0.00164 and 0.00179, the mean being 0.00172.

The agreement of these two values is not particularly good, though not exceeding the probable error given above.

For some unrecorded reason this specimen was tested again, and gave—

From D and H', $x = 0.00174$

„ C and H', $x = 0.00170$

Mean value = 0.00172

The susceptibility was not in this case computed from mixtures D

and C taken together, for that would introduce extrapolation, which we avoid as much as possible.

In the case of the three specimens of susceptibilities greater than 0.01 we are compelled to extrapolate beyond our strongest mixture, and with the eighteen specimens below 0.001 we make our weakest mixture a factor in the extrapolations.

In some instances (*e.g.*, No. 45) there is a very distinct difference in the strength of two pieces broken off the same hand specimen.

A few of the specimens used in the earlier investigations were re-examined, and gave considerably larger susceptibilities than those previously measured. This is probably due to an error in the determinations of the absolute value of the susceptibilities of the liquids employed, but in the case of any individual rock this error would not exceed the difference often observed between two different fragments of the same specimen. The fact also that the mean value now obtained is almost exactly the same as that formerly given (*viz.*, 0.00255 as against 0.00245) justifies the conclusions based upon the use of this or the smaller number (0.00160) which was used in many of the calculations on the magnetic effects of basaltic rocks. The accuracy of the absolute values now given has been tested by experiments on ferric chloride. Although the susceptibility of this substance is much smaller than that of most of the rocks, the result of our measurements was in close accord with the means of values deduced from the experiments of Quincke and Townsend.

Table of Results.

Number.	Numerical description on list sent by Sir A. Geikie.	Nature.	Locality.	Susceptibility of		Mean susceptibility.
				1st specimen.	2nd specimen.	
1 to 12.—Rocks from Northumberland, Cumberland, and Durham.						
1	E 1348 108	Basalt-dyke	½ mile E. of Otterburn Church, Northumberland	0·00835
2	E 1349 —	Basalt-dyke	Near coal working, 1½ miles N.E. Folstone Church, North Tyne, Northumberland	0·00172
3	E 1352 —	Basalt-dyke	300 yards E. of Middle Riding, and 1½ miles N.E. of Elsdon, Northumberland	0·01008	0·01009	0·01009
4	102	Diabase or dolerite	Cunsfell, 2½ miles S.E. Melmerby Church, Cumberland	0·00020
5	..	Olivine dolerite	½ mile N.E. Melmerby Church, Cumberland	0·00058
6	108	Basalt	Westerly continuation of Acklington Dyke, S. of Hindhope, Cheviots	0·00447	0·00338 (readings rather discordant)	0·00390
7	E 1369 109 N.W.	Basalt	Whin Sill, Rugley, near Alnwick, Northumberland	0·00455	0·00447	0·00451
8	110 S.W.	Basalt	Whin Sill, Belford Quarry, Northumberland	0·00289	0·00263	0·00280
9	E 896 a 102 S.E.	Dolerite	Whin Sill, Sleight Edge, W. of Wemmergill, Lunedale, (?) Teesdale, Durham	0·00359	0·00331	0·00345
10	E 690 —	Dolerite	Whin Sill, Middleton, Teesdale, Durham..	0·00265
11	(?) 103	Basalt-dyke	Bowleis Beck, Durham	0·00230	0·00221	0·00225
12	101	Gabbro	(Boas), White Crag, Carrock Fell, Cumberland	(a very small piece)	(a very small piece)	0·00023

13 to 20.—*Rocks from Wales, Stafford, and Salop.*

13	E 618	78	Dolerite	The Skerries, Anglesey	0·00680
14	E 887	78 N.W.	Basalt	Llanfarchell, Anglesey	0·00213
15	E 1916	56	Gabbro	Hanter Hill near Kingston, Radnor	0·00007	0·00007	0·00007
16		40	Gabbro	St. David's Head	(a very small piece)	0	0
17		62	Olivine-dolerite	Rowley Regis, Staffs.	0·00454
18	E 2006	55	Olivine-dolerite	Clee Hills near Ludlow, Salop.	0·00445
19	E 2168	—	Olivine-dolerite	Butterton Hall Park, Staffs. (Post-Triassic)	0·00229	0·00244	0·00236
20		19	Olivine-dolerite	Stoke Lane quarry, Mendip Hills	0·00111

21 to 25.— <i>Rocks from Devon and Cornwall.</i>							
21	1039	21 or 26	Olivine-basalt	Radden Court quarry, N. Devon	0·00021	0·00022	0·00022
22		22	Dolerite	N. of railway cutting, N. of Ashton St., Devon	0·00027	0·00034	0·00031
23		22	Dolerite	Crocombe, Thrusham Station, Devon	0·00027
24		32	Gabbro	Kildown Cove, Cadgwith, Cornwall	0·00113	0·00029	0·00071
				(a very coarse-grained specimen)	(mainly felspar)	(mainly diallage, (?)	
25	Hornblende-schist	Quarry near Church Cove, Lizard	0·00028

26 to 44.— <i>Tertiary Rocks from Atrim. 45 from Devon.</i>							
26		7	Dolerite	New quarry, back of railway station, Portrush	0·00229	0·00207	0·00218
27	J.R. 232	—	Basalt	(Upper sheets), Giant's Causeway	0·00157	0·00160	0·00159
28	222	—	Basalt	(Upper sheets), Brocky Set quarry, Ballin- toy	0·00295	0·00284	0·00290
29	212	—	Basalt	(Upper), Carrick-a-Raidhe	0·00137
30	234	—	Basalt	(Lower sheets), Giant's Causeway	0·00123	0·00124	0·00123
31	208	8	Dolerite	Intrusive, resting on headland, Murlough Bay, S.E. of Fairhead	0·00276	0·00264	0·00265
32		—	Dolerite	Segregation vein in the preceding	0·00420
33	107	—	Basalt	Intrusive in carboniferous, in stream 1½ miles E. of Ballycastle	0·0154 (?)	0·0124 (?)	0·01390 (?)

Table of Results—continued.

Number.	Numerical description on list sent by Sir A. Geikie.	Nature.	Locality.	Susceptibility of		Mean susceptibility.
				1st specimen.	2nd specimen.	
34	167	Basalt	(Lower sheets), Garron Point	0·00026(?)	0·00036	0·00031(?)
35	20	Basalt	Scawt Hill, 4 miles S.S.E. of Glenarm	0·00302
36	92	Basalt	(Lower sheets), immediately above trachyte, N. slope of Brown Dod Hill	0·00115	0·00067	0·00091
37	243	Trasalt	(Upper), ½ mile N.W. of Slemish Mountain	0·00034	0·00031	0·00033
38	118	Basalt	(Lower tabular trap), near Anlarp Head, 1½ miles S.E. of Glenarm	0·00051
39	122	Basalt	(Upper sheets), quarry by road, 1 mile S. of Glenarm	0·00085
40	22	Basalt	(Neck), quarry S. side of Carnmoney Hill	0·00308
41	29	Basalt	(Plateau, flow), quarry, Carnmoney Hill..	0·00019
42	64	Basalt-dyke	Near Tardree	0·00188	0·00177	0·00183
43	98	Basalt	(Lower trap), E. side of Dongon Hill, 1 mile N. of Park Gate	0·00061	0·00075	0·00063
44	33	Basalt	(Overlying ash bed), quarry by railway, Ballypaddy, 2 miles N.E. of Temple Patrick	0·00082
45	294	Diorite-dyke, epidiorite, or protobase	Rostravor, co. Down (post-silurian)	0·00659	0·01028	0·00843

The mean susceptibility of 1 to 12 (Northern Counties) is 0·00340
 " 13 " 20 (Welsh, &c.) 0·00269
 " 21 " 25 (Devon and Cornwall) 0·00086
 " 26 " 45 (Antrim) 0·00255
 ∴ Mean of 25 English rocks 0·002559
 " 20 Irish rocks 0·003546
 Mean of all 0·002553

"On the Detection and Localisation of Phosphorus in Animal and Vegetable Tissues." By A. B. MACALLUM, Associate-Professor of Physiology, University of Toronto. Communicated by Professor SHERRINGTON, F.R.S. Received June 15,—Read June 16, 1898.

The distribution of phosphorus, like that of organic iron, in tissues, is a question of considerable importance to the cytologist and it is therefore necessary that the method of detection for this element, should be a satisfactory one. There are difficulties, however, which make the micro-chemical detection of phosphorus less easy than in the case of iron, for there is no precipitate holding phosphorus which, under the microscope, gives as striking a demonstration of its presence as ferrous sulphide does of iron. Ammonium phosphomolybdate is, in the test-tube, a markedly coloured precipitate, but when its constituent crystals are examined under the microscope the colour observed counts for little. When also, as in tissues, the precipitate may be in a much more finely divided form, the canary-yellow colour may be so faint that it is indistinguishable from the yellow produced in the tissue by the action of the nitric acid in the precipitating reagent, although Jolly* holds that the yellow colour of the phospho-molybdate compound in the tissue cannot be simulated by dilute nitric acid.

To get over these difficulties Lilienfeld and Monti† used pyrogallol to reduce the molybdic portion of the compound to the condition of a lower oxide after they had, by washing the preparations in water, removed the uncombined molybdate of ammonia from the tissues. "Pyrogallol gives, in the test-tube with phospho-molybdic acid, an intense colour varying from brown to black, whereby lower oxides of molybdenum arise."‡ In speaking in another place of the action of pyrogallol on the phospho-molybdate, they state that it gives, in the parts of the preparations rich in phosphorus and according to the quantity of the latter, "a yellow, brown, or black colour."

Račiborski§ points out that the reaction of pyrogallol with ammonium phospho-molybdate in the test-tube is a green one, while that produced with ammonium molybdate is a brown one. This author further states that the green reaction is obtained in the tissues of *Euphorbia* wherever crystals of ammonium phospho-

* "Contribution à l'histoire biologique des phosphates," 'Comptes Rendus,' vol. 125, p. 538, 1897.

† "Ueber die mikro-chemischen Localisation des Phosphors in den Geweben," 'Zeit. für Physiol. Chemie,' vol. 17, p. 410, 1893.

‡ *Loc. cit.*, p. 411.

§ *Vide* a criticism of Lilienfeld and Monti's observations, 'Bot. Zeit.,' vol. 51, p. 245, 1893.

molybdate occur, but a brown colour in other parts of an intensity which varies according to the length of time during which the preparation is washed, but if it is long and continuously treated with water no brown colour appears. The brown, therefore, would be due to molybdate of ammonium, and is no indication of the presence of the phosphorus compound.

Heine* was unable to confirm Raçiborski's observations regarding the reaction produced by pyrogallol, but he found, using stannous chloride as a reducing agent, that almost invariably a blue reaction appeared, which may pass eventually into a dirty green colour. In the test-tube also the reaction with the reducing agent is, according to the amount of the molybdate present, as well as to the strength of acidity in the fluid, a green, brown, or blue one, whether phosphates are present or not.

Pollacci,† using zinc chloride as a reducing reagent, found the resulting colour range from dark blue to grey.

It is evident from the foregoing that there is error somewhere in the observations which have been made on the action of pyrogallol on ammonium phospho-molybdate, and it is obvious that, if Raçiborski is right in his contention, then the results of the investigations of Lilienfeld and Monti, relying as they did upon the "yellow, brown, or black" reaction to indicate the presence of phosphorus, must be wrong. As a number of observers, including Sherrington,‡ Gourlay,§ and Held,|| have used the same method and the same criteria on special tissue elements, it is therefore important to know the truth concerning the results so obtained.

My observations confirm Raçiborski's on the action of pyrogallol on ammonium phospho-molybdate. When the former, in solution, aqueous or ethereal, is allowed to act on the thoroughly washed phospho-molybdate precipitate, the canary yellow of the latter is invariably turned to green, even in the presence of nitric acid, and this colour is maintained for a couple of hours, after which the precipitate takes up slowly a darker shade, until at the end of twenty-four hours it has a black colour with a faint shade of green in thin layers. The form of the crystals, which are black, is maintained. When the

* "Ueber die Molybdänsäure als mikroskopischer Reagens," 'Zeit. für Physiol. Chemie,' vol. 22, p. 132, 1896-97.

† "Sulla distribuzione del fosforo nei tessuti vegetali," 'Malpighia,' vol. 8, 1894; Abstract in 'Zeit. für Wiss. Mikrosk.,' vol. 11, p. 539.

‡ "Note on some Changes in the Blood of the general Circulation consequent upon certain Inflammations of acute and local character," 'Roy. Soc. Proc.,' vol. 55, p. 161, 1894.

§ "Proteids of the Thyroid and Spleen," 'Journ. of Physiol.,' vol. 16, p. 23, 1894.

|| "Beiträge zur Structur der Nervenzellen und ihrer Fortsätze," 'Arch. für Anat. und Phys., Anat. Abthg., 1895, p. 396.

reducing reagent is allowed to act on a nitric acid solution of ammonium molybdate, a brownish-black or black colour is produced, and an amorphous precipitate may be formed, which, under the microscope has a grey blue-black appearance, the fluid itself remaining brown, the colour being due to the oxidised pyrogallol. At the end of twenty-four hours the amorphous elements are black, with or without a brown shade. When, on the other hand, pyrogallol acts on ammonium molybdate in solution the resulting colour is deep brown, very much like that of a saturated solution of Bismarck brown, which is maintained at the end of twenty-four hours, but in no case is a precipitate formed. It must be noted in this case that the colour formed results immediately on the addition of the pyrogallol.

That what happens in the test-tube is what also obtains in tissues may be shown readily. If one impregnates sections of tissue with ammonium molybdate for an hour or more, these, thoroughly washed and then treated with a pyrogallol solution, give a brown colour, which is most marked in the parts of the cells which have an affinity for colouring matters. It is obvious that in the absence of nitric acid there is no phospho-molybdate compound present, and yet the reducing reagent shows that though repeated washings were resorted to, the ammonium molybdate has not been removed. On the other hand, when the tissues are placed in a nitric acid solution of ammonium molybdate the results obtained are strikingly different. One may conveniently demonstrate these by placing fresh *Spirogyra* threads in the solution for from five to ten hours at a temperature of 35—40° C., then washing them quickly in distilled water and putting them in a freshly prepared strong aqueous solution of pyrogallol. In ten minutes the threads may be again washed, dehydrated, cleared in oil of cedar, and mounted in balsam. Wherever in such preparations one expects to find phosphorus, *e.g.*, in the nuclei, it is demonstrated by the green reaction. If the pyrogallol is allowed to act longer than ten minutes it begins to stain the cells and to mark the green more or less with a brown coloration, which distributes itself in them as colouring matters generally do.

Perhaps the most striking way of demonstrating that the phospho-molybdate is turned green and the molybdate brown by the action of pyrogallol, is by impregnating portions of thin strips of writing paper with a solution of sodic phosphate, drying them, and then submitting them to the action of the nitric-molybdate solution, which gives them a yellow colour. On now washing them in distilled water, and submitting them to the pyrogallol solution, the areas which are impregnated with the phospho-molybdate become green in a few seconds, while the parts which contain the molybdate solution alone become brown or yellowish-brown, and the contrast between the two reactions thus appears marked.

The error at the base of the process adopted by Lilienfeld and Monti has been the assumption that it is possible to wash out of tissues all traces of the ammonium molybdate not combined with phosphoric acid. I have found that when the stamens of *Erythronium americanum*, treated for twenty-four hours with the nitric-molybdate solution, were washed with distilled water many times for five months, they gave, at the end of that time, marked evidence of the presence in them of ammonium molybdate. The addition of stannous chloride brought out everywhere in such preparations the appearance of the blue molybdic oxide, whereas, when such preparations were treated with pyrogallol solution, the phospho-molybdate compound was found to be limited in its distribution. From animal tissues also, I have found it impossible to remove by washing the molybdate reagent. Indeed, one may succeed thus in removing the phospho-molybdate compound rather than the other.

Heine* also has specially insisted on the strong affinity that cell substances, those which contain phosphorus as well as those which do not, have for ammonium molybdate, forming with the latter, compounds insoluble in water or in dilute nitric acid solutions. He prepared a quantity of histon, free from phosphorus compounds, which, after treatment with the nitric-molybdate reagent and after frequent washings, gave with stannous chloride abundant evidence of the presence of ammonium molybdate.

One is consequently justified in concluding that the results of Lilienfeld and Monti's observations, based as they are on the "yellow, brown, or black" reaction of the pyrogallol, are incorrect, and that while the reaction may appear in phosphorus-holding elements, it is simply a coincidence, and not an indication of the presence of phosphorus.

The property of pyrogallol to form, in the reduction of the molybdate and the phospho-molybdate compounds, a coloured substance which can be taken by cellular elements just in the same way and to about the same extent that they take up other colouring matters in solution, constitutes an objection to the use of this reducing reagent. It is not possible to be certain in all cases in regard to the length of time during which it is to be allowed to act, and, consequently, a very faint green may be obscured by a light brown reaction, resulting either from the oxidised pyrogallol or from the reduced molybdate in the presence of traces of nitric acid.

In consequence of this objection, I endeavoured to find a reducing reagent which would leave the molybdate compound, in the presence of nitric acid, unaffected, while it would markedly react with the phospho-molybdate, not only in the test-tube, but in tissues. Zinc

* "Ueber die Molybdänsäure als mikroskopisches Reagens," 'Zeit. für Physiol. Chemie,' vol. 22, p. 132, 1896-97.

chloride does this, but in an unsatisfactory way. It is very slow in its action, and feeble in its reducing power. It gives a green reaction with the phospho-molybdate compound, but none with the molybdate in the presence of nitric acid. Stannous chloride reduces both the compounds at once, forming the blue oxide of molybdenum, and therefore it is, for the point in view, valueless. Ferrous sulphate is also very slow in its action, and it has the disadvantage of giving a faint green colour to the tissue, independent of that which may be produced in the phospho-molybdate compound.

The reagent which I found the most valuable from every point of view is phenylhydrazin hydrochloride. This, in an aqueous solution of 1—4 per cent. strength, is certain in its action if it is freshly prepared or not more more than two or three days old. It, in the absence of alcohol or of a caustic alkali, makes a very marked distinction between the molybdate and the phospho-molybdate compounds. It gives with the former, in powder, the brown oxide at once, in solution, a brownish precipitate which may or may not appear immediately, depending on the strength of the solution, but in a solution of the molybdate containing nitric acid, *e.g.*, that used as the reagent for phosphoric acid, it has no apparent effect on the molybdenum compound, although, in a few minutes, a soluble, reddish, aromatic compound may be formed in the solution. On the other hand, with phospho-molybdates, either in the presence or in the absence of ammonium molybdate, or nitric acid, or of both, it gives at once the dark-green oxide of molybdenum. Examined under the microscope, the crystals of the phospho-molybdate alone are found to have the green colour, which, after an hour's action of the phenylhydrazin, is so dark as to suggest, at first sight, black. That this reaction depends upon the presence of phosphoric acid, may be clearly shown by adding to a mixture of the reducing reagent and of the nitric molybdate solution a quantity of phosphoric acid solution. Although the mixture will stand for several minutes without any change other than the formation of a slightly reddish solution, yet on the addition of the acid solution the dark-green reaction appears immediately and markedly, sometimes accompanied by the formation of a blue-violet soluble compound. No other acid exercises a like effect. Solutions of potassium hydrate and sodic hydrate and alcohol, in a certain proportion, will call forth in the mixture a greenish-blue or blue colour, which, in the case of the alcohol preparation, fades out in twenty-four hours. In this latter, the colour would appear to be due to the formation of an aromatic compound, and not directly to an alteration in the molybdate. Nitric acid alone will produce, in a solution of phenylhydrazin, a reddish colour, and rarely also, when ammonium molybdate is present, a blue-violet colour, which appears to be due to a phenylic

compound. What the conditions are, under which this coloured compound is produced, have not been determined, but this reaction cannot interfere with or confuse the results of the action of the reducing reagent on the phospho-molybdate compound.

On the molybdate and phospho-molybdate compounds distributed in animal and vegetable tissues, the phenylhydrazin hydrochloride acts as it does on these in the test-tube. It is not necessary to free the tissue preparations from ammonium molybdate. They may be placed for a minute or two in a dilute solution of nitric acid, after which they are transferred to the reducing solution, which, in less than two minutes, brings out the green colour where the phospho-molybdate compound occurs, but a faint yellow reaction where ammonium molybdate alone is present. Instead of dilute nitric acid, one may use distilled water, but it is not necessary to do even this, for if the preparations are transferred directly to the reducing fluid with but what may adhere to them of the nitric-molybdate solution, the result is the same.

When the reducing fluid has been allowed to act for the proper length of time the preparations are washed in distilled water, then dehydrated, cleared in oil of cedar, and mounted in balsam. Preparations made in this way four months ago are now quite as satisfactory as they were at first.

Reference to the other reagents and methods which have been used is also necessary. The nitric-molybdate reagent was made by dissolving one part of pure molybdic acid (MoO_3) in four parts of strong ammonia, and adding thereto, slowly, fifteen parts of nitric acid, sp. gr. 1.2. The proportions indicate weights. The resulting solution had a faint yellow tinge, and, after decantation from the very slight sediment, remained free from a precipitate as long as any of it was unused.

Fresh tissue material was used as well as that which had been hardened in alcohol. The alcohol material is the best, for the nitric acid, before it converts the phosphorus compounds in fresh tissue elements into orthophosphoric acid, must dissolve a portion at least of the phosphorus-holding proteids, and thus the phosphorus when converted may not be distributed as *intra vitam*. I have, however, used fresh material, wherever possible, to compare with that hardened in alcohol. The latter offers advantages in the fixed form of the elements, and in the preparation of thin sections which readily permit the uniform action of the reagent as well as the extraction of lecithin and inorganic phosphates.

The time during which the reagent was allowed to act on the preparations varied from ten minutes to twenty-four, and even, in some cases, forty-eight hours. It was found that a temperature of 35°C . favoured considerably the formation of the phospho-molybdate. The

formation is a progressive one, the extent of the reaction appearing to have some relation to the time employed. The inorganic phosphates are first affected, then lecithin, the organic phosphorus being much more slowly converted into the orthophosphate.

According to Liebermann,* the phosphorus found in such compounds as nuclein and nucleic acid is in the form of monometaphosphate, but Kossel† has thrown doubt on the results on which this view is based, and he claims that the facts point rather to the occurrence of other anhydrous forms of phosphoric acid in these compounds. Jolly‡ has inferred from his experiments that in organic compounds of phosphorus the latter does not occur in the unoxidised metalloid ("metalloïdique non oxydé intégré") form. Milroy§ has found that in the digestion of nuclein compounds with trypsin, some of the phosphorus is set free as orthophosphoric acid, but the greater part (89·08—91·63 per cent.), occurring in an organic form, does not possess the characters of metaphosphoric acid, for its solutions may be boiled a long time without producing an increase in the amount of the ortho compound present.

As the nitric-molybdic reagent reacts only with the ortho form of phosphoric acid, it is obvious that the organic phosphorus in the tissues must be put in the condition of orthophosphoric acid. Lilienfeld and Monti treated the fresh tissues with baryta water or sodic carbonate, in order to set the phosphorus free as phosphate, which was then demonstrated as the phospho-molybdate; but, as Liebermann|| points out in the case of yeast nuclein, the baryta compound is only after long boiling, or, after heating with acids, converted into the orthophosphate. The action of the baryta must, in part at least, be to change the structure of the elements, and it is not certain, therefore, that in all cases the ortho compound formed should be in the structures where the phosphorus originally existed. This, and the fact that the sodium compound first formed by sodic carbonate, being soluble, may diffuse from its original situation, render this method of doubtful value in localising phosphorus in tissue elements. These observers, however, claim that the nitric acid in the molybdic reagent has the property of gradually converting the phosphorus compounds into the orthophosphate, and they allowed fresh prepara-

* "Nachweis der Metaphosphorsäure im Nuclein der Hefe," 'Arch. für die ges. Physiol.,' vol. 47, p. 155, 1890.

† "Ueber die Nucleinsäure," 'Verh. Physiol. Gesell. zu Berlin,' 'Arch. für Anat. und Physiol.,' Phys. Abth., 1893, p. 157.

‡ "Recherches sur le phosphore organique," 'Comptes Rendus,' vol. 126, p. 531, 1898.

§ "Ueber die Eiweiss-Verbindungen der Nucleinsäure und Thyminsäure und ihre Beziehung zu den Nucleinen und Parannucleinen," 'Zeit. für Physiol. Chemie,' vol. 22, p. 307, 1896-97.

|| *Loc. cit.*

tions to remain a long time in this fluid for this purpose. I have, as already stated, found that the long continued action of the reagent has this result, and that the conversion is more marked if the reagent is allowed to act at a slightly increased temperature. One cannot be absolutely certain that the anhydrous forms of phosphoric acid when liberated, and before being converted into orthophosphoric acid, do not diffuse through the tissue elements, but in a number of experiments made to decide this point, I ascertained that if such diffusion did occur, it was in such minute amounts as to be unobservable. A risk of diffusion is incurred when a tissue, very rich in orthophosphates, is acted on by the reagent. A part of the phosphoric acid in this case, except in very thin sections, diffuses and forms a slight deposit of phospho-molybdate crystals on the preparation. Preparations of renal tubules and the cat's placenta illustrate this well.

Owing to the abundance and general distribution of lecithin in animal and vegetable tissues, it is necessary to extract this compound from them in order to ascertain the distribution of the other phosphorus compounds. Bitto* has shown that the extraction can be regarded as complete only when the material, first treated with ether, has been acted on by boiling ethyl alcohol thirty times, each period of extraction lasting about ten minutes. Adopting this process, I subjected samples from all the material used to extraction in a Soxhlet apparatus for five hours, the condensed but still hot alcohol being siphoned off every 6—10 minutes. This treatment is specially necessary in the case of nerve tissues in which it makes a marked difference in the phospho-molybdate reaction.

A much more difficult problem is that of the removal of the inorganic phosphates from tissues. Jolly† used acetic acid of 20 per cent. strength for this purpose, claiming that this fluid removes all the phosphates except that of iron. It does indeed remove a large part of them, but not those which may be in the nuclear elements. In order, therefore, not to confuse the inorganic phosphorus with that of organic combinations, I have always endeavoured to determine in any given material what extent of molybdo-phosphate reaction may be obtained in the first ten minutes after the nitric-molybdate reagent is added. This reaction indicates whether the tissues are rich or poor in inorganic phosphates, and it may be compared with what may be obtained after a longer stay in the reagent, any enhancement in the reaction thus demonstrating the phosphorus of organic compounds.

* "Ueber die Bestimmung des Lecithingehaltes der Pflanzenbestandtheile," *Zeit. für Physiol. Chemie*, vol. 19, p. 488, 1894.

† 'Comptes Rendus,' vol. 125, p. 538, 1897.

Results of the Method.

I. *General*.—The chromatin of all nuclei gives, after eighteen hours' treatment with the nitric-molybdate reagent a strong phospho-molybdate reaction. This is so marked that the nuclei appear under ordinary microscopic magnification as if they were stained with a dark-green dye for the express purpose of showing the chromatin structures. Even the finer fibrils constituting the so-called reticulum are prominently brought out. This is well illustrated in the nuclei of the epithelial cells of the skin, alimentary tract, renal tubules, and olfactory region, and of the muscle fibres, liver cells, testicular and ovarian cells, nerve cells (spinal cord), pancreatic cells, connective tissue cells, and leucocytes of *Menobranchus (Necturus) lateralis* and *Amblystoma punctatum*. In vegetable cells, as shown in *Erythronium americanum*, the same result was found. In brief, wherever true chromatin was found, there the reaction for phosphorus was obtained. In the chromatin of the mitotic loops in dividing animal and vegetable cells, no reaction more marked than in the chromatin of the resting nuclei was in any case obtained. This fact definitely contradicts the view of Lilienfeld* that the chromosomes in mitosis are composed of nucleic acid only, a view which Heine,† as a result of experiments in staining with mixtures of dyes, also rejected. The phosphorus in nucleic acid amounts to 9–10 per cent., but in nuclein it is 3–4 per cent. If Lilienfeld's view is correct, then the reaction for phosphorus in the chromosomes should be at least twice as marked as in resting chromatin elements, taking volume for volume. The results obtained by Lilienfeld in his staining experiments must be explained on some other hypothesis than that which he adopted.

The eosinophilous nucleoli in animal and vegetable nuclei give a strong reaction for phosphorus, but less marked than in the case of the chromatin. On the other hand, the nucleolar elements in the nucleus of the ovary of *Erythronium* which, as I have pointed out,‡ are rich in "masked" iron, give a deep reaction for phosphorus. A similar result was obtained in the nucleoli of the nuclei of the embryo-sac of the same form, in the peripheral nucleoli of the maturing ovarian ova of *Menobranchus*, in the nucleoli of *Corallorhiza multiflora* and of *Spirogyra*, all rich also in "masked" iron. The

* "Ueber die Wahlverwandschaft der Zellelemente zu gewissen Farbstoffen," 'Verh. Berl. Physiol. Gesell.,' 'Arch. für Anat. und Phys.,' Phys. Abth. 1893, p. 391.

† "Die Mikrochemie der Mitose, zugleich eine Kritik mikrochemischer Methoden," 'Zeit. für Physiol. Chemie,' vol. 21, p. 494, 1895–96.

‡ "On the Distribution of Assimilated Compounds of Iron, other than Hæmoglobin and Hæmatins, in Animal and Vegetable Cells," 'Quart. Journ. Micr. Sci.,' vol. 38, p. 175, 1895.

nucleoli of the nerve cells in the spinal cord of *Menobranchus* and of the ox and dog, give a deep reaction, but it is not uniform throughout the nucleolus, portions of a granular form, giving a deeper colour than the surrounding material.

In the cytoplasm of various cells the organic phosphorus present is usually small in amount, and, unless inorganic phosphates are present, the lecithin being extracted, the reaction is a very faint one. In the cells of the nucellus and in the bast cells of *Erythronium* a deeper reaction is obtainable in the cytoplasm; and this appears to be due to the presence of chromatin—at least in the case of the nucellar cells. The cytoplasm of the latter is also, as I have pointed out elsewhere, iron-holding. Other exceptions are found in the pancreatic cells, liver cells, nerve cells, striated muscle fibres, in maturing and mature ovarian ova of Amphibia, and in the spermatozooids of *Ascaris*. These exceptions are referred to at greater length below.

In dividing cells the achromatic spindle gives no reaction for phosphorus. This result is quite the opposite of that which Heine obtained when he used stannous chloride as a reducing reagent after the employment of the nitric-molybdate reagent. Heine advanced the view that his result showed that the molybdic reagent could not be depended on to indicate the presence of phosphorus in tissues. It is rather to be interpreted as indicating that stannous chloride does not distinguish between the molybdate and the phospho-molybdate compounds.

In no case has the centrosome or centrosphere in animal and vegetable cells given a reaction for phosphorus.

II. *Special*.—The zymogen granules in the pancreas of *Diemyctylus*, from which the lecithin was thoroughly extracted, gave a deep reaction for phosphorus after eighteen hours' treatment with the nitric-molybdic reagent. The phosphorus apparently is less firmly bound than is the case in the nuclear chromatin in the same cells, for the reaction in the latter is slower in appearing. A very distinct but less deep reaction was obtained also in the protoplasm in which the granules lie, more especially in the part adjacent to the lumen, and a marked reaction also was produced in the antecedent substance of the zymogen, found usually in the outer or protoplasmic zone of the cells. This substance, which I have named prozymogen,* contains iron in a "masked" form, and it stains in every way like chromatin. The presence of phosphorus, as well as of "masked" iron, seems to indicate very clearly that it is a nucleo-proteid.

The demonstration that zymogen and prozymogen are phosphorus-

* *Loc. cit.*, p. 224; also "Contributions to the Morphology and Physiology of the Cell," 'Trans. Can. Inst.,' vol. 1, p. 247, 1891.

holding confirms the view which I advanced seven years ago,* that both are primarily derived from the nuclear chromatin.

The deeper reaction for phosphorus which is obtained in that part of the pancreatic cell immediately adjacent to the lumen, may be due to ferments dissolved in the cytoplasm at this point or to a phosphorus-holding substance derived from the zymogen at the same time the ferments are formed.

A diffuse reaction for phosphorus, slow in appearing, was obtained in the cytoplasm of liver cells of dog and man. These cells also frequently contain abundance of inorganic phosphates whose presence may render the demonstration of the organic compound difficult.

I have been unable to determine whether organic phosphorus compounds are present in the cytoplasm of the renal cells, for in the dog and human subject these cells are rich in inorganic phosphates which are difficult to extract, and, consequently, obscure the reaction for the other compounds if these occur here.

Mr. F. H. Scott, who is at present working on the micro-chemistry of nerve cells, has found that Nissl's granules also give a distinct reaction for phosphorus. He has also found that the substance forming the granules does not digest in artificial gastric juice. Mackenzie† had previously found that these granules contain "masked" iron. They stain like chromatin. These facts lead one to conclude that the substance of the granules is a nucleo-proteid.

A feeble reaction for phosphorus has been obtained in the axis cylinders of medullated nerves from which the lecithin was extracted.

In the muscle fibres from the chelæ of the crayfish a deep phosphorus reaction was obtained in the dim bands and in the beadlets which constitute Dobie's line, while no reaction occurred in the lateral discs of Engelmann. The phosphorus-holding substance is coterminous with the anisotropic element. The phosphorus demonstrated is not due to presence of lecithin, for this was wholly extracted from the preparation before it was treated with the nitric-molybdic reagent, and it was not due to inorganic phosphates, for the reaction did not come out, except very feebly, during the first twenty minutes.

In the striated muscle fibres of Amphibian larvæ the iron-holding substance appears to be also limited in its distribution, as it was found only in the dim bands,‡ Dobie's line giving no evidence of its presence, perhaps because this structure in Amphibia is too minute to permit a proper determination of this point. In my experiments on crayfish muscle both the dim band and Dobie's line appear to give

* 'Trans. Can. Inst.,' vol. 1, p. 247, 1891.

† "Investigation in the Micro-chemistry of Nerve Cells," 'Brit. Assoc. Report,' 1897, p. 822.

‡ 'Quart. Journ. Micr. Sci.,' vol. 38, p. 220.

a reaction for "masked" iron, and thus in muscle this element and phosphorus would seem to have the same distribution.

The matrix of cartilage in *Menobranchus* and the frog gives a marked reaction for phosphorus, which seems in large part to be due to inorganic phosphates, for it appears soon after the addition of the nitric-molybdate reagent. The reaction in some specimens appears in areas or zones about cartilage cells or groups of them, the areas being separated by narrow zones in which no reaction was observed.

In the maturing and mature ovarian ova of *Amphibia* the cytoplasm is very rich in organic phosphorus, though not so much so as the nucleus. As the yolk spherules form, the amount of phosphorus-holding substance seems to lessen, possibly through its being taken up by the spherules which, even when freed from traces of lecithin, give a marked phospho-molybdate reaction in about six hours. It is to be noted that these spherules are also iron-holding.

In the spermatozooids of *Ascaris* the organic phosphorus is, on the whole, distributed as I have found the "masked" iron to be in these structures.* The "nucleus" gave a deep phospho-molybdate reaction, and a less marked reaction was obtained in the surrounding cytoplasm.

A diffuse but distinct reaction for phosphorus was obtained in human chorionic villi of the seventeenth (?) day, and in the placental villi of the sixth week and third and sixth months. A part of this reaction is due to inorganic phosphates, for it is obtained to a certain extent in about ten minutes after the nitric-molybdic reagent is added. The cat's placenta is very rich in inorganic phosphates distributed throughout the tissue, but more abundant in the deeper portions of the organ.

The colloid bodies of the thyroid are phosphorus-holding according to Gourlay,† who relied in his experiments on Lilienfeld and Monti's interpretation of the action of pyrogallol on the phospho-molybdate compound. Through the kindness of Dr. J. H. Elliott, I obtained an abundance of free colloid bodies of the ox, fixed in alcohol, which, after extracting the lecithin, I fused in a platinum cup with crystals of pure potassic nitrate. The mass, treated with a quantity of the nitric-molybdate solution, became yellowish, owing to the formation of the phospho-molybdate, the characteristic crystals of which could be found under the microscope. The reaction was not due to inorganic phosphates, for when thin sections of the ox's thyroid, freed from lecithin, were placed in the reagent, the phospho-molybdate compound formed very slowly, and the maximum reaction appeared only after eight hours. The presence of organic phosphorus in these elements does not, as Gourlay

* 'Quart. Journ. Micr. Sci.,' vol. 38, p. 229.

† *Op. cit.*

believes, necessarily indicate the existence of a nucleo-proteid in them, for Dr. Elliott has found that they digest in artificial gastric juice, leaving no residue, which would not be the case were a nucleo-compound present.

The outer portions of the rods and cones in *Menobranchus* and *Diemyctylus* are rich in organic compounds of phosphorus. It is more abundant in the rods than in the cones, and it is not due to lecithin, for the retinæ used were freed from the latter, nor is it owing to the presence of inorganic compounds of phosphorus, for the reaction is not obtainable during the first twenty minutes after placing the organs in the nitric-molybdate solution, while it is a progressive one up to the sixth hour. The chromatin of the nuclei of all the layers of the organ also gives the reaction.

The chromatophore in *Spirogyra* gives a weak phospho-molybdate reaction, and it appears to be due to the presence of an organic compound of phosphorus. A more marked reaction, however, is usually found in the pyrenoids in the same genus, and also in those of *Edogonium*, *Cladophora*, and *Conferva*. In fresh specimens of *Spirogyra*, taken during daylight and put into the nitric-molybdate reagent, the pyrenoids appeared to give a stronger reaction than those of specimens taken at ten o'clock at night. The reaction develops slowly.

A diffuse reaction for phosphorus, slow in developing, was obtained in the cytoplasm of *Saccharomyces Ludwigi*. In apparently normal cells this may be the only reaction which will be obtained, but in cells cultivated in the sap of the iron-wood tree a spherical body occurs, at first sight like a nucleus, but frequently homogeneous, which after about ten hours' treatment with the nitric-molybdate reagent gives a reaction for phosphorus which may be very marked. This body is in no sense a nucleus,* nor does the phospho-molybdate reaction reveal any structure that corresponds to the latter. The fact that the "masked" iron in these cells has a distribution parallel to that of the organic phosphorus, also points distinctly to the absence of a nucleus.

In Cyanophyceæ the "central body" always gives evidence of the presence of organic phosphorus compounds. A stronger reaction for phosphorus was obtained in the iron-holding, chromatin-like granules which are to be found in the central body, or on its periphery, in *Tolypothrix* and *Oscillaria*. The "cyanophycin" granules, on the other hand, have not given any evidence of the presence of organic phosphorus except in some few filaments of a preparation of *Oscillaria tenuis*, in which case a marked reaction was developed in about an hour.

* I have discussed the nature of this body, 'Quart. Journ. Micr. Sci.,' vol. 38, p. 246.

FALMOUTH MAGNETIC OBSERVATORY.

IMPORTANT NOTE.

The observations made at this Observatory during 1897 have not been printed at their usual place in the 'Proceedings,' as the Dip observations for the period between October 8, 1895, and June 5, 1897, inclusive, have been found to be affected with certain errors.

OBITUARY NOTICES OF FELLOWS DECEASED.

Dr. HUBERT A. NEWTON, Professor of Mathematics in Yale University, whose death occurred on the 12th of August, 1896, was born in 1830, on the 19th of March, at Sherburne, in the State of New York. Both his parents were descended from ancestors who were among the first British settlers in Connecticut. His father built the Buffalo section of the Erie Canal, and it is recorded of his mother, whose maiden name was Butler, that she was remarkable for her mathematical attainments. He was one of ten children—seven sons and three daughters.

At school the lad showed the aptitude for mathematics, and especially for geometry, which distinguished him throughout his life. He entered Yale University at the age of 16, and graduated with the highest mathematical honours in 1850. After his graduation he continued for two and a half years more to devote himself to the study of advanced mathematics, at the expiration of which time he was, in 1853, appointed mathematical tutor in his university. Two years later, at the unusually early age of 25, he was elected to the full professorship, which he held through the rest of his life. In 1859 he married a daughter of the Rev. Joseph C. Stiles, who survived her husband only three months, leaving two daughters.

Professor Newton's life was one of great industry. He was Associate Editor of the 'American Journal of Science' for twenty-seven years, was a member and afterwards President of the Publishing Committee of the Connecticut Academy of Arts and Sciences, and, in addition to a long list of original memoirs, wrote articles for various cyclopædias, among others for the 'Encyclopædia Britannica.' He took an active part in promoting the introduction of metric measures into America, and on the Board of Management of the Yale Observatory, which owed its existence largely to the efforts and personal sacrifices of Professor Newton, and of which he was for a long time Secretary and for two years Director. He even took a part in municipal affairs, and it is characteristic of the esteem in which he was held, that it is recorded of him that he was elected alderman in a ward in which the prevailing politics were in opposition to his own. In 1875 he presided over the Mathematical Section of the American Association for the Advancement of Science, and in 1885 was President of the Association. At an early period he received the honorary degree of LL.D. from Michigan University, and in 1888 was

awarded the Smith Gold Medal of the American National Academy of Sciences, in recognition of his original work. In this country he was elected a Foreign Member of the Royal Society, of the Royal Astronomical Society, and of the Royal Society of Edinburgh.

The first of his papers seems to have been published in 1857, and the last, "On the Relation of the Plane of Jupiter's Orbit to the Mean Plane of 401 Minor Planets," in 1895. Between these dates he published a long series of papers—usually from two to four each year—covering a variety of subjects in mathematics, insurances, and especially in that branch of astronomy which relates to meteors and comets. These intimately connected phenomena early fixed his attention. His first paper in reference to them was published in 1860, and a continuous succession, nearly fifty in all, have been the result of his studies in this department of astronomy, and have contributed largely to the immense advance which the astronomy of meteors has made within the last forty years.

Two memoirs may be selected to illustrate how much modern science owes to Professor Newton's industry and clear insight. The first of these is his great memoir entitled "The Original Accounts of the Displays in former times of the November Star-shower: together with a Determination of the length of its Cycle, its Annual Period, and the probable Orbit of the Group of Bodies round the Sun." This memoir is published in the 'American Journal of Science' ('Silliman's Journal'), vols. 37 and 38 (1864). In it Professor Newton makes use of the collections of ancient records of star-showers which had been brought together chiefly by the great industry of French antiquarians and French astronomers. From these records Professor Newton traces out all which refer to former visits to the earth of that great swarm of small bodies which are now known as Leonids, but which, when first observed, radiated from the constellation Cancer. In each case he cites the actual words of the original records, of which there are usually several referring to each shower; and by a careful scrutiny of these he is able to fix, in many instances with certainty, in others with more or less probability, the actual date on which each shower occurred, and even in some cases the hours during which it lasted. He thus discovered that we possess records of thirteen showers of these meteors, of which the earliest was in A.D. 902, and the last (at the time when he wrote this memoir) was in 1833. To these we have now to add the two great displays witnessed from Europe in 1866, and from America in 1867.

By this careful scrutiny Professor Newton discovered several important facts—that the main swarm returns to the earth at intervals of 33.25 years; that on each return the earth encounters the dense part of the swarm in two consecutive years; that the date of the

principal showers has advanced at a nearly uniform rate from October 12, old style, which was the date in A.D. 902, to November 12, new style, which was the date in 1833; and finally that the meteoric orbit, whatever it is, is but little inclined to the ecliptic, and that the motion of the meteors where they enter the earth's atmosphere is nearly perpendicular to the direction of the sun.

Such being the facts, he proceeds to determine what inferences may be drawn from them. From the dates of the showers he ascertained that the node of the meteoric orbit—the point of its intersection with the earth's orbit—has been since A.D. 902 advancing in longitude nearly uniformly and at the average rate of $1.711'$ annually. Allowing for the precession of the equinox, this is equivalent to an advance of $29'$ in $33\frac{1}{4}$ years, measured from a fixed point. This motion is accordingly direct, and Professor Newton infers from this and from dynamical considerations that the motion of the meteors in their orbit must be retrograde. He next considers whether the meteoric orbit is wholly or only partly occupied by the dense swarm of meteors. He first examines the hypothesis of an elliptic orbit along which the meteors are distributed uniformly, and which suffers such perturbations that it shifts about so as periodically to intersect the earth's orbit three times in a century. He finds that this hypothesis must be rejected, because it involves an apsidal motion so rapid as would require perturbing forces of an intensity which we can satisfy ourselves do not exist. Accordingly the meteors, leaving out of account the sporadic meteors which have got separated from the main swarm, occupy only a portion of their orbit. He next inquires what further can be learned about an orbit of which the main swarm of meteors occupies only a portion; and he made the important discovery that only five orbits are compatible with the observed return of the swarm to the earth at intervals of $33\frac{1}{4}$ years. One of these five, accordingly, must be the true orbit. Professor Newton determined the periodic times in these orbits, and thus ascertained the axis major of each. All that was then wanting to fix the precise form and position of each of the five orbits was a sufficiently accurate determination of the "radiant point," i.e., of that direction from which the meteors are seen to enter our atmosphere. On account of Professor Newton's representations, efforts were made by astronomers to make this observation with the utmost care during the great meteoric showers of 1866 and 1867. This direction, when corrected for the deflection of the meteors by the earth's attraction, furnishes the position in space of one tangent to the orbit. Knowing then the focus, the axis major, and the position and point of contact of one tangent of each of the five orbits, its exact form and situation in space can be ascertained. Thus the five orbits become fully known; and the next step was to determine which of them is the

actual orbit of the meteors. Professor Newton pointed out a line of investigation by which it was possible that this discrimination might be made. Bodies revolving round the sun in these several orbits would be differently acted upon by the surrounding planets. The perturbations in these five orbits would accordingly be different, and would probably lead to a different rate of shift of the node of the orbit along the plane of the ecliptic. If then, the perturbations in all the five orbits can be so fully investigated that the rate of the shift of the node in each can be computed, it will then be seen which of the five computed amounts accords with that which Professor Newton deduced from the observations, viz., an advance of $29'$ in $33\frac{1}{4}$ years, or $50\cdot2''$ annually.

This was an invaluable suggestion, and the key to the complete solution of the problem, although there was at the time little hope that any mathematician could be found competent to grapple with the difficulties of the problem, which involved the investigation of a kind of perturbation which had never been attempted, viz., the perturbation by a planet of a body revolving in the reverse direction in an orbit round the sun, which is nearly coincident with the orbit of the planet. Fortunately our own Professor J. Couch Adams was able to cope with all the difficulties of the problem, and after five months' labour found himself in a position to make known which of the five orbits is the real orbit of the meteors. This marvellous achievement, however, would not have been possible without the discoveries that had been made by Professor Newton: 1st, of the amount of the average shift of the node; 2nd, of the fact that the choice lay between five orbits which he defined; and 3rd, that a discrimination between these was theoretically possible by the method afterwards successfully employed by Adams. Thus one of the most marvellous discoveries of the century in dynamical astronomy is due to the associated efforts of these two great men. Professor Newton indicated the problem and pointed out how it was to be attacked, and Professor Adams successfully grappled with its immense difficulty.

One other example of Professor Newton's contribution to our knowledge of astronomy must here suffice. It may be treated briefly. Most comets come into that portion of space which is occupied by the solar system from great distances outside. Such comets move either in parabolas, or in ellipses or hyperbolas which approximate to parabolas. By an examination of the orbits of 247 comets, Professor Newton establishes the fact that the planes of the orbits of these non-periodic comets lie in all positions indifferently, and that such comets exhibit no preponderance of direct over retrograde motion; whereas all the known periodic comets, which are about fifteen in number, move in planes which are but moderately inclined to the *planes in which the principal planets move*, and show such a marked

preponderance of direct over retrograde motion that only two have their motion retrograde, viz., Halley's comet and the comet associated with the great Leonid swarm of meteors.

Professor Newton succeeded in explaining this remarkable difference in behaviour of the two classes of comets. He shows that the preponderance of small inclinations and the preponderance of direct over retrograde motions would inevitably establish themselves amongst comets of short period, on the supposition that each of these is a comet of the other class which has at some time passed so close to a planet that it was drawn aside from its original orbit.

Laplace had shown that if a comet passes close to a planet the influence of the planet upon it may be found to a first approximation by drawing a sphere of a certain size round the planet, and supposing that the comet has moved in a parabola round the sun, undisturbed by the planet, until it passes inside the sphere; and that while inside the sphere it moves in a hyperbola relatively to the planet, attracted by the planet alone and unperturbed by the sun. This is equivalent to supposing that as a first approximation we may neglect the small difference between the direction and amount of the sun's attraction upon the comet and planet while the former is traversing the sphere from the point of its entrance into the sphere to its point of exit.

Professor Newton points out that if the comet passes in front of the planet as the planet advances along its orbit, then it will necessarily accelerate the planet and thereby increase the planet's kinetic energy. An equal amount of energy must be lost by the comet, of which therefore the speed relatively to the sun decreases; and therefore, if the orbit round the sun was a parabola before it entered the sphere of the planet's influence, it will start along an ellipse after emerging from that sphere. It thus becomes a member of the solar system. On the other hand, if the comet pass behind the planet the opposite effect is produced. The planet loses kinetic energy which the comet gains, so that the comet when it extricates itself from the sphere of the planet's influence, proceeds to move in a hyperbolic orbit round the sun, past which it can make but one sweep, and will then finally quit the solar system unless it encounter some other planet.

Professor Newton deals specially with the planet Jupiter. It is manifest that the only parabolic orbits which approach that planet are to be found amongst those of which the perihelion lies as near to the sun or nearer than the orbit of Jupiter. Professor Newton shows that out of 1,000,000,000 comets traversing the solar system in such orbits, about 839* will have their orbits changed by that

* From this number a small deduction, perhaps of some dozen or so, has to be made, to allow for those comets which actually collide with the planet.

planet into elliptic orbits with a periodic time less than that of Jupiter; and by a further scrutiny of the dynamical conditions he finds that moderate inclinations to the orbit of Jupiter will largely preponderate among the comets so affected, and that direct motions will preponderate over retrograde—thus explaining both these observed facts of nature. This remarkable investigation will be found in three papers, one in the ‘American Journal of Science and Arts,’ 3rd Series, vol. 16 (1878), and the other two in the Reports of the British Association for 1879 and 1891.

It is recorded of Professor Newton that he was noted in his own university for the special pleasure which he took in all mathematical investigations upon which geometrical insight could be made to bear; and it must strike every student of Professor Newton’s published work that science in large measure owes the discoveries which he made to the clearness of his geometrical and dynamical conceptions, and to his facility in dealing with them.

This record ought not to close without referring to the circumstance that Professor Newton’s original researches were the offspring of his leisure. He regarded the duties of his professorship as those of primary obligation upon him; to these he at all times first gave his full attention, and he seems to have possessed in a conspicuous degree the powers of imparting to the students who had the good fortune to be brought into contact with him a share of his own enthusiastic love of mathematics. The motives which impelled him to devote in addition the time which he felt to be at his own disposal to a search into the secrets of nature, are illustrated by words that he once used and which will find an echo in many minds:—“To discover some new truth in nature, even though it concerns the small things in the world, gives one of the purest pleasures in human experience, and it gives joy to tell others of the treasure found.”

G. J. S.

RICHARD QUAIN, who died on March 13, 1898, at the age of 81, was born on October 30, 1816, at Mallow-on-the-Blackwater, co. Cork, in which county his family was one of the best known and most respected. His father, John Quain, was a younger brother of Richard Quain, of Rathenhy, whose sons, Jones and Richard, were distinguished for their knowledge of anatomy and surgery, and John Richard as a lawyer and judge in the Court of Queen’s Bench. The father of the subject of this notice married, in 1815, Mary, daughter of Michael Burke, of Mallow, a member of an ancient and honoured Irish family.

After early education at Cloyne, Richard Quain was apprenticed to a *medical practitioner* in Limerick, where he acquired a knowledge of

many of the essentials of medical practice. In 1837 he entered the University College of London, where his two cousins were, the one Demonstrator, the other Professor of Descriptive and Practical Anatomy, from whom he seems to have received much sympathy and valuable instruction. In this School of Medicine he studied with much diligence, and his perseverance and keen powers of observation obtained for him many distinctions.

In 1840 he graduated as M.B. of the University of London, obtaining high honours in physiology, surgery, and midwifery. He continued to gain much experience in the appointments of Resident Surgeon or Physician at the hospital, and in 1842 he obtained the degree of M.D. at the London University, receiving a gold medal and certificate of special proficiency. He was soon afterwards elected a Fellow of University College.

In 1848 he became Assistant Physician to the Hospital for Diseases of the Chest, at Brompton, where he was associated with Drs. Walsh, Theophilus Thompson, and Cotton. In 1855 he was elected Physician to this hospital, and his connection with it as a Consulting Physician continued till the time of his death. He was also Consulting Physician to the Seamen's Hospital at Greenwich, and to the Consumption Hospital at Ventnor.

In 1851 Quain was elected a Fellow of the Royal College of Physicians, and was identified with it till the time of his death; for he was a member of the Council, Censor, Lumleian Lecturer, Senior Censor in 1877, Harveian Orator in 1885, and Vice-President in 1889. In 1888, on Sir William Jenner's retirement, he contested the Presidency with Sir Andrew Clark, who, however, was elected, though only by eight votes, in a large meeting.

In 1863 Quain was elected as Crown nominee of the Medical Council, and continued in that post till his death. He was a moving spirit in all the work of that body; he was a member of many committees, serving with great distinction on the Pharmacopœia Committee, which he seemed to make his special care, though most active on several others. The services he rendered to this Council in the various offices he held were most valuable, and the result was his appointment, on the death of Mr. John Marshall in 1891, to the post of President, to which he was unanimously re-elected on the expiry of his first term of office in 1896, when he gave a valuable address, clearly setting forth the questions in which the Council were interested and his own practical and statesman-like view of the methods of dealing with them. His predecessors in this important office were Sir Benjamin Brodie, Joseph Henry Green, Sir George Burrows, Sir George Edward Paget, Sir Henry Acland, and John Marshall, none of them more devoted to the duties or more efficient as President of the Council than himself.

Sir Richard Quain's literary work and his researches into various departments of medical science were, if not numerous, very important. As a member of the Royal Commission appointed in 1865 to consider the question of rinderpest or cattle plague, in which he was associated with Lord Spencer, Lord Cranborne (now Marquess of Salisbury), Lord Sherbrook, Dr. Lyon (now Lord) Playfair, Dr. Edmund Parkes, and Dr. Bence Jones, he took a prominent part, and was an earnest advocate of the stamping-out measures recommended by the 'Commission, which, though strongly opposed at the time, subsequent events have proved to have had the result of saving large sums of money to the nation. He was a frequent contributor to the 'Saturday Review,' to the 'Lancet,' and other medical journals; whilst his treatise on "Fatty Degeneration of the Heart" in the 'Transactions of the Medical and Chirurgical Society' for 1850, expanded into a more elaborate article in his 'Dictionary of Medicine' some years later. His reports, in conjunction with the staff of the Brompton Hospital, compiled for several years, of the cases treated there; some valuable contributions to the 'Lancet' of 1845 on Bright's disease, and to the 'Edinburgh Monthly Journal of Medicine' on "Injuries of the Valves of the Heart," together with his Lumleian Lectures given before the College of Physicians in 1872 on "Diseases of the Muscular Walls of the Heart" were, and are still, regarded as authoritative writings.

But the great work with which Quain's name will ever be associated is that of the 'Dictionary of Medicine,' on which the years between 1875 and 1882 were spent, and which reappeared in a second edition in 1894, enlarged and brought up to the knowledge of the present time. For this cyclopædia of medical science he had carefully selected the contributors from the most eminent members of the medical profession, whose communications were all revised and, in some cases, modified by himself. His own contributions, especially those on "Fatty Degeneration of the Heart," "Angina Pectoris," "Aneurism of the Heart," "Diseases of the Bronchial Glands and General Remarks on Disease" are not the least valuable. The work, in short, having filled a want long felt by the profession, gained their entire confidence. To his able coadjutors, Dr. Frederick Roberts, Dr. Mitchell Bruce, and Mr. John Harold he gave due credit, and to their untiring devotion to the work its success is in great part—as he himself would have acknowledged—to be attributed.

Not the least interesting of Quain's contributions to medical literature was his Harveian Oration, delivered before the Royal College of Physicians in 1885, in which he dealt eloquently with the healing art in its historic and prophetic aspects.

In 1871 Dr. Quain was, for his eminence as a physician and for



Thomas Joseph Hyland.

Portrait by Mrs. J. H. Hyland, 1892. (From the collection of the Royal Society.)

scientific research into subjects connected with medicine, elected a Fellow of the Royal Society. He was also a member of the Senate of London University elected by the Queen, LL.D. of Edinburgh, M.D. (Hon.) of Dublin and of the Royal University of Ireland, and also a Fellow (Hon.) of the Royal College of Physicians of Ireland. He was Fellow and President of both the Medical and Chirurgical and the Pathological Societies, to the 'Transactions' of which he made several valuable contributions, and member and President of the Harveian Society of London.

In 1890 he was appointed Physician Extraordinary to the Queen; and on New Year's Day, 1891, received the well-merited honour of a baronetcy of the United Kingdom. This becomes extinct with his death, as Sir Richard Quain leaves no son. Isabella Agnes, Lady Quain, to whom he was married in 1854, was the only daughter of Mr. George Wray, of Cleasby, Yorkshire she died, to his profound grief, a few months after the baronetcy had been conferred upon him. Four daughters survive him.

Sir Richard Quain was much and justly esteemed by his profession and by the public. The kind-heartedness and geniality of his nature, his amusing and epigrammatic conversation, his wide knowledge of men, and his unwearying sympathy and kindness, made him popular not only with the younger as well as the older members of his profession, but with society generally, and in the Athenæum and Garrick Clubs, of which he was a well known member, whilst the bright and cheering effect of his presence in the sick room was always beneficial. Few men have been more endowed with the faculty of endearing themselves to their acquaintances, friends, and patients; and few will be more regretted than the warm-hearted, genial Irishman and physician who has been taken from us, though not until advanced age had afforded the world full opportunity of appreciating his merits.

J. F.

JAMES JOSEPH SYLVESTER was born in London on September 3, 1814. He was the youngest son of Abraham Joseph, and had five brothers and two sisters. His eldest brother early in life established himself in America, and assumed the name of Sylvester; an example followed by all his brothers.

James went to Neumegen's well-known Jewish boarding school at Highgate from the age of six until he was twelve. Mr. Neumegen, a good mathematician, was strongly impressed by the boy's mathematical talent, sedulously fostered it, and sent him at the age of eleven to be tested by Dr. Olinthus Gregory at the Royal Military Academy at Woolwich. Dr. Gregory, after examining in algebra, pronounced him to be possessed of great talents, and recommended his mathematical tutor to pay great attention to his instruction. He

subsequently went to Mr. Daniell's school at Islington for a year and a half, where he came under Mr. Downes. Thence to London University for five months, and in 1829, being then fifteen years of age, he was sent to Liverpool. During this time Dr. Gregory kept up his interest in the boy, for, in March, 1827, he wrote to his father "Pray have the goodness to drop me a line so soon as your son returns home that I may endeavour to fix a day in which I may have the pleasure of seeing him here, and tracing his progress since I saw him before."

At about this period he was for a short time a pupil of De Morgan. At Liverpool he went to the Royal Institution,* which had itself been founded in 1814, and its school in 1819. He lived with his aunts, who had a school in Duke Street almost opposite Colquill Street at the corner of Cornwallis Street, the house then commonly known as "Morrell's Folly." One of his aunts was married to Elias Moxley, one of three brothers who represented Barnard's Bank in Lord Street. It appears from the school records that at a meeting of proprietors on the 12th February, 1830, Thomas Langton, Esq., president, Sylvester was awarded the prize in the second class as the result of the examination after the winter vacation. Other prize-winners were William Robertson Sandbach, George Hancox, and Murray Gladstone. In regard to the mathematical prize secured by Sylvester, it is stated "In the Mathematical School one of the students, who had previous to entering it attained considerable proficiency, was so far advanced before the other scholars that he could not be included in any class; the first prize has without competition been awarded to him." At the presentation of prizes, Mr. Langton addressed him in the following words:—"In presenting to you, James Sylvester, the youngest of the successful students, the first prize for mathematical progress, let me caution you not to look upon it as an occasion of triumph; in receiving it you are giving a pledge of your future diligence in the improvement of those natural abilities with which a kind Providence has blessed you." The pupils of the school numbered but thirty. The Rev. T. W. Peile, B.A., Fellow of Trinity College, Cambridge (afterwards head master of Repton), was head master; William Moore, B.A., of Trinity College, Dublin, second master; and Mr. Marratt, mathematical master.

Few now remain who were with him at this school. Of these Sir William Leece Drinkwater, until quite recently first deemster, Isle of Man, perhaps knew him best, and I am indebted for many particulars to a letter which he has been good enough to write me. It does not

* Mr. John Forshaw, who went to the school shortly after Sylvester left, writes that the Mechanics' Institution, originally founded in 1825, has since been transformed into the Liverpool Institute, which must not be confused with the Liverpool Royal Institution standing about a quarter of a mile away in Colquill Street.

appear that Sylvester was in all cases kindly treated by his school-fellows. He was kind-hearted and brave, but rendered extraordinarily irritable by the constant references, in a spirit of opposition, to his Jewish extraction. He fought many battles in defence of his religion with, it is said, greater courage than skill. On at least one occasion he wished to fight a duel, being aware that with fists he was no match for his opponent. At one time, considering that he was treated with oppression by one of the under-masters, and being directed, with the rest of the class, to write a theme on "Despotism," he composed an excellent essay, giving various instances, both high and low, of the abuse of power, but reflecting unmistakably upon the case of the under-master and himself. He concluded:—"Thus we see that power begets tyranny, whether in the case of the mightie * monarch or of the petty usher of a school or institution." The severe punishment that followed, it is related, was endured with great courage.

Soon after, Sylvester ran away from the school. He sailed from Liverpool, and shortly found himself in the streets of Dublin with but a few shillings in his pocket. This led to a singular incident. As the boy was walking down Sackville Street he was observed by an elderly gentleman who, his curiosity aroused, stopped and inquired into his circumstances. A few moments' conversation sufficed to reveal the fact that the boy was related to his wife, being, in fact, her first cousin! Thereupon he invited him to his house, entertained him, and finally sent him back to Liverpool. The gentleman was the Right Hon. R. Keatinge, Judge in the Prerogative Court of Ireland.

Being fully aware of his great knowledge of mathematics, he was in the habit of proposing questions beyond the capacity of the mathematical master. Before leaving he gained a prize of five hundred dollars offered from the United States for the solution of a certain question. It seems that a problem in combinations of great difficulty had come under the notice of a certain D. V. Gregory, a friend of Sylvester's elder brother in New York. On the advice of the latter it was sent to the younger brother in Liverpool, who almost immediately solved it. Its nature may be gathered from the subjoined extracts from a letter addressed to him a few years afterwards by D. V. Gregory:—"You solved my problems, which I submitted without their knowledge, to the great satisfaction of the Contractors of Lotteries in this country, and they expressed, frequently, an exalted opinion of your mathematical attainments in solving so intricate a subject. The inventor of the combination system himself was never able, as I learn, to package by any mathematical rule . . . On account of their withdrawing from business at the end of this year, the managers had prepared all

the necessary printing to complete their engagements, which printing was done according to the making up of packages by boys in our employ without any order or system or mathematical arrangement. This was a tedious process, and required some months' labour and consequent expense, and terminated in making a great number of miscellaneous packages containing a disproportion of numbers. Had your mathematical skill been known when they commenced business in 1823, or even five years after, and had they adopted your arrangement, they would have saved thousands of dollars expended by them in preparing for the printer."

Sylvester was less than two years at the Liverpool Institution. Afterwards he read for a few months with the Rev. Richard Wilson, D.D. (late Fellow of St. John's College, Cambridge), and then in 1831, at the age of seventeen, was entered at St. John's College, Cambridge. He came out first in his first year; but in June, 1833, he became seriously ill and had to remain at home till November. He then returned to the University, but again unfortunately became ill in February, 1834, and was obliged to remain at home for nearly two years, not rejoining his college till January, 1836. In the following month he had the misfortune to break a blood-vessel. On recovering, he pursued his studies till January, 1837, when he came out Second Wrangler. Griffin was Senior of the year, and the list contained also the name of Green.

Being unwilling to sign the Thirty-nine Articles, he was unable to take a degree, to obtain a Fellowship, or to compete for one of the Smith's prizes.

At this time, on the occasion of laying the foundation stone of the Mechanics' Institution, Mount Street, Liverpool, Sylvester presented Lord Brougham with his pamphlet criticising Euclid's definition of a straight line as length without breadth. He also composed his first paper on "Fresnel's Optical Theory of Crystals," which appeared in vol. 11 of the 'Philosophical Magazine;' and on the death of Dr. Ritchie in the same year he became a candidate for the Chair of Natural Philosophy in the London University College. The testimonials which he received for that occasion are evidence of the high estimation in which he was held by tutors, examiners, and the other scientific men with whom he had been brought into contact. The list of his supporters includes the names of J. W. Heaviside (the Senior Moderator in 1837), S. Earnshaw (Senior Examiner), George Peacock, W. H. Miller, H. Philpot, J. Hymers, W. Hopkins, J. W. Colenso, P. Kelland, J. Bowstead, J. Cumming, Frederick Thackeray, James Hildyard, E. Bushby, Richard Wilson, J. Challis, and Olinthus Gregory. Evidently all these were aware that a star of the first magnitude was rising in the mathematical firmament. They seem *particularly* to have noticed his analytical power and command of

language, combined with originality and enthusiasm: qualities which were conspicuous throughout all his subsequent scientific career. It may be observed too that his interest was not confined to the subject of his greatest predilection, for whilst at Cambridge he attended regularly the chemical lectures of Cumming and the classical lectures of Bashby. This catholicity of taste, so early exhibited, is doubtless one reason for the brightness and freshness with which, throughout life, he could treat the dullest and most abstruse subjects.

He was appointed to the Chair at University College in the session 1837-38, his friend De Morgan holding the Chair of Pure Mathematics. He had some difficulty in drawing diagrams on the black-board to illustrate his lectures. He was, in fact, never clever with his hands, his handwriting in particular being very bad. A curious instance of his constant desire to be thorough is brought to light by the circumstance that for some time after taking up the professorship he took lessons in drawing from the college drawing master; it is said, however, with small results. He published a remarkable series of papers in the 'Philosophical Magazine,' vols. 13 to 17, principally on matters connected with the Theory of Equations, Elimination, Sturm's Functions, &c., and laid the foundations of the work with which his name will ever be associated. He retired from University College in the session 1840-41, and immediately afterwards accepted the Professorship of Mathematics in the University of Virginia. For the due appreciation of matters that will be presently related, it should be stated that Sylvester at this period felt strongly on the subject of slavery, and was, moreover, in the habit of expressing himself thereon with great warmth. He was indeed antagonistic to oppression in all its forms.

In the United States 1840 was the presidential year. It was the eve of the introduction of new political methods. A new party was formed, with the platform "Absolute and unqualified divorce of the general government from slavery, and the restoration of equality of rights among men." Feeling ran high, particularly in Virginia, which was, later on, one of the Confederate States. Men of experience warned Sylvester that he should, on crossing the water, be guarded in his expressions, and refrain from hotly stating his opinions on the subject of slavery. He, however, determined to go, and, after sitting for a full length portrait in oils, by Patten, of the Royal Scottish Academy, now in possession of the family, he embarked at Liverpool in a Cunard sailing vessel. The portrait is evidently the work of a good painter, and is stated to be an excellent likeness. It represents a young man of six and twenty, in cap and gown, with dark, curly hair, and spectacles, seated, book in hand, at a table.

In America he appears to have been at war with his surroundings from the first. He found nothing sympathetic or inspiring, and the cause of his exit from the country after six months arose from an unfortunate incident with two students in his own class.

For two or three years after his return from Virginia he appears to have done little work. As remarked by Dr. Halsted there were distinct periods of his life during which he felt much discouraged, and seemed to have no heart for mathematical research.

In 1844 activity recommenced. He was elected to the post of Actuary to the Legal and Equitable Life Assurance Company. This was a responsible post, particularly at that time when, through mismanagement, one of the principal establishments in England had been brought to the brink of ruin. He made constant valuations and acted as check officer and scientific adviser to the directors of this and some other companies for many years, residing for the greater part of the time at 28, Lincoln's Inn Fields. He also accomplished an extraordinary amount of mathematical research. A few titles of the papers now published during this time will give a general idea of the subjects which principally occupied his mind:—"On the Dialytic Method of Elimination," "Elementary Researches on the Analysis of Combinatorial Aggregation," "On a discovery in the Theory of Numbers relative to the Equation $Ax^3 + By^3 + Cz^3 = Dxyz$," "On the Rotation of a Body about a Fixed Point," "Sketch of a Memoir on Elimination, Transformation, and Canonical Forms," "On the Principles of the Calculus of Forms," "On the Expressions for the Quotients which appear in the Applications of Sturm's Method to the discovery of the real Roots of an Equation." A number of these papers refer to the subject now known as the theory of invariants. It rose from its foundations, which had been partially laid by Boole in 1844, under the strong hands of Cayley and Sylvester. The conception of the problem and much of its orderly development may be ascribed to the former, whilst nearly the whole of the nomenclature and a great deal that is now recognised as being of capital importance, both as regards initiation and brilliant extension, is due to the latter. During the decade from 1845 he established his position as one of the foremost mathematicians of Europe. He had the friendship and esteem of such men on the continent of Europe as Lejeune-Dirichlet, Poncelet, Borchardt, Duhamel, Bertrand, Serret, Hermite, Otto Hesse, Peters, Kummer, Richelot, Joachimsthal, Chasles, with many of whom his correspondence was frequent and voluminous. The contemporaries in his own country—William Rowan Hamilton, Ivory, De Morgan, Graves, MacCullagh, John Herschel, Babbage, Donkin, Challis, Kelland, Salmon, William Thomson, and others—also testified on occasions that they were aware a great mathematical genius had

appeared in the ranks of scientific men, and was rapidly forcing his way to the front.

In 1854, on the death of W. M. Christie, Sylvester was a candidate for the Professorship of Mathematics at the Royal Military Academy at Woolwich. Christie had been professor since 1838, preceded by Gregory, 1821-38; Bonnycastle, 1807-21; Hutton, 1773-1807; Cowley, 1761-1773; Simpson, 1743-1761.

Leading mathematicians at home and abroad testified to his eminence and fitness for the appointment, but, as shown by the subjoined letter, he was not successful:—

“ From Lieut.-Colonel Portlock,
“ Royal Military Academy, Woolwich,
“ 1st August, 1854.

“ Sir,

“ I am directed by the Lieutenant-Governor, Major-General Lewis, C.B., to notify to you that the Lieutenant-General of the Ordnance has selected the Rev. Matthew O'Brien to succeed Mr. Christie as Professor of Mathematics in this establishment.

“ In making this notification, I feel it due to you to state that the great weight of your claim as a candidate was felt and recognised.

“ I have the honour to be,

“ Sir,

“ Your obedient servant,

“ J. PORTLOCK,

“ Lieut.-Col. Inspector.”

“ J. J. SYLVESTER, Esq.”

Owing to the destructive fire of 1873 there is now no record of this letter in the archives of the academy.

One cannot help recalling the rejection by the same establishment, a century before, of the celebrated Benjamin Robins, Copley medallist of the Royal Society, in favour of a Mr. Müller.

Mr. O'Brien was known as a fair mathematician, and had previously held the post of Lecturer on Physical Science. He did not take up the appointment, as a few months after his election his death occurred, when Sylvester was elected. In the interval, however, he sought, but did not obtain, election to the vacant Professorship of Geometry in Gresham College, and delivered a probationary lecture on geometry before the Gresham Committee on December 4, 1854. The lecture was printed, and in the preface occur the following characteristic remarks:—“ The author will only so far forestall the arrival of the period (*quod longum absit!*) above alluded to by protesting against the use of the word ‘ practical ’ as employed by an ingenious lecturer who succeeded him at the desk. To discourse fluently on things of practice is no suffi-

cient evidence in itself of a practical mind. The first rule of practice is to do all things at the right time and in their proper places; to proportion the means to the ends and the ends to the means; above all to know what is possible, and to confine one's endeavours within the limits of the feasible. The author allows, and has habitually acted on the principle, that for the purpose of *illustrating* lectures on geometry or any other abstract science, the lecturer should lay his hands on the plough, the loom, the forge, the workshop, the mine, the sea, the stars, all things on earth or under heaven which may help to arouse the attention or interest the imagination of his auditors. But to profess to make the mere applications of a science such as geometry the staple of the matter to be taught within the walls of the college by the Gresham lecturer, to undertake to comprise within a course of geometrical lectures systematic instruction in mechanics, astronomy and navigation, descriptive geometry, engineering and drawing, the method of interpolation, the theory of toothed wheels, the two kinds of perspective, machinery, mapping, the art of shipbuilding, rules for cutting the best form of screws, and for enabling the citizens of London to qualify themselves for being their own land surveyors, is a suggestion which, with all due deference to its propounder, the author regards as one of the wildest and most visionary which ever entered into the mind or issued from the lips of a practical man." The address, composed at short notice, is a powerful essay on geometrical science.

He took up the appointment of Professor of Mathematics and Lecturer in Natural Philosophy at the Royal Military Academy on the 15th September, 1855. In August, 1856, the lectureship was taken over by the Professor of Practical Astronomy. The salary of the appointment was £550 per annum combined with a Government residence, medical attendance, and right of pasturage on the common. He occupied K Quarters, Woolwich Common, being the last of a long list of residential professors. The house was commodious and with a good garden. There he frequently entertained his friends from London and distinguished foreign mathematicians. At the same time he always had chambers in London in the neighbourhood of the Athenæum Club. These he had taken originally with the intention to practise at the Bar. Scientifically this was a glorious period for Sylvester, for, seated under a walnut tree which grew in the centre of his garden, he made some of the great discoveries with which his name will be for ever associated. He wrote about eighty papers, and naturally it is only possible here to glance at a few of those which are of fundamental importance. During 1857-58 he published remarkable advances in the theory of the Partition of Numbers, and in 1859 delivered seven lectures on the subject at King's College, London. The outlines of these dis-

courses have this year (1897) been published for the first time by the London Mathematical Society; they have attracted considerable attention, and have already led to a remarkable paper by Mr. G. B. Matthews, F.R.S.

In these researches Sylvester, standing upon the shoulders of Cauchy, showed how to form an algebraical expression, involving the imaginary roots of unity of different orders for the general coefficient in the associated generating function. It was a piece of analytical skill that could only have proceeded from a mind endowed with imagination of the highest order.

In 1864 appeared in the 'Philosophical Transactions of the Royal Society' a paper which will perhaps be considered his greatest achievement. The title is "Algebraical Researches: containing a Disquisition on Newton's Rule for the Discovery of Imaginary Roots, and an allied Rule applicable to a particular class of Equations, together with a complete Invariantive Determination of the Character of the Roots of the General Equation of the Fifth Degree, &c." Newton had given in the 'Arithmetica Universalis' a rule for discovering an inferior limit to the number of imaginary roots in an equation of any degree, but without proof or indication of method or marshalling of evidence. Maclaurin, Campbell, Euler, and Waring had also treated the question, but either failed to obtain a solution or had fallen into serious error in the attempt. Sylvester's memoir, described by him as a Trilogy, falls into three parts; in the first he establishes Newton's rule in regard to algebraical equations as far as the fifth degree inclusive; in the second he obtains a rule applicable to equations of the form

$$\Sigma(ax+b)^m = 0,$$

m being any positive integer, and a, b real coefficients; in the third he determines the absolute invariantive criteria for ascertaining the exact number of real and imaginary roots appertaining to an equation of the fifth degree. Here, as in his treatment of the Partitions of Numbers, he has frequently resorted to geometrical intuition. In the present investigation every superlinear function is conceived to be in association with a pencil of rays constructed in a definite manner, and much of the argument is given in the language of the geometry of pencils. During a conversation with the writer in the last weeks of his life, Sylvester remarked as curious that notwithstanding he had always considered the bent of his mind to be rather analytical than geometrical, he found in nearly every case that the solution of an analytical problem turned upon some quite simple geometrical notion, and that he was never satisfied until he could present the argument in geometrical language.

During these years he continually wrote upon the theory of inva-

riants, making important additions to it. The facility with which he associated subjects the most diverse is evidenced by the titles of some of his papers. Thus, "Thoughts on Inverse Orthogonal Matrices, Simultaneous Sign Successions, and Tessellated Pavements in two or more Colours, with Applications to Newton's Rule, Ornamental Tile Work, and the Theory of Numbers;" and "Astronomical Prolusions, commencing with an Instantaneous Proof of Lambert's and Euler's Theorems, and modulating through the Construction of the Orbit of a Heavenly Body from two Heliocentric Distances, the Subtended Chord, and the Periodic Time, and the Focal Theory of Cartesian Ovals, into a Discussion of Motion in a Circle and its Relation to Planetary Motion."

Particular events occurred during this period, which should be mentioned. About the year 1855 the 'Quarterly Journal of Mathematics' was founded, and Sylvester, who took a chief part as Editor, was anxious to have a suitable motto for the title page. He consulted many of his friends on the matter, De Morgan amongst others, and finally, after much correspondence, selected the following:—

· "ὅτι οὐσία πρὸς γένεσιν, ἐπιστημὴ πρὸς πίστιν
καὶ διάνοια πρὸς εἰκασίαν ἔστι."

This motto appeared upon the title-page during the whole time that he was editor. The trouble he took in this matter is evidence of his interest in things which may appear trivial to others, but which, being important in his own eyes, he spared no effort to accomplish successfully. His mathematical correspondence was of wide range, and with De Morgan, Cayley, Salmon, Hermite, and Charles he exchanged letters continuously. Cayley for a long time resided at 2, Stone Buildings, Lincoln's Inn, and frequently met Sylvester; indeed, they were in the habit of taking long walks, Cayley walking from London and Sylvester from Woolwich, meeting near Lewisham. It is certain that some of the fruits of these long consultations are before the world to-day.

It cannot be said that he was a great success at Woolwich as a teacher, being too far beyond his pupils who, for the most part, regarded mathematics as an irksome duty. He had the reputation of being eccentric and irritable. When not actually engaged in teaching, the mind of Sylvester would occasionally become abstracted from earthly affairs, and it is stated that on one occasion he suddenly looked up from a paper in the hall of study and demanded of the corporal on duty, "What year is it?" An explosion of laughter in the room led to a "scene," and the subsequent infliction of many punishments upon the cadets.

The sight of Sylvester leaving his house pursued by his landlady carrying his collar and necktie is said to have been not an unusual

one. He came into collision on more than one occasion with the authorities at the Academy and with the War Office. The culmination of these disputes was in 1870, when a War Office enactment, abolishing the separate offices of Professor of Mathematics and Professor of Mechanics, coupled with a limit of age for the new appointment, forced him to retire. At first it was the intention to make him leave without a pension, but, through the strenuous exertions of his friends in and out of Parliament, it was finally determined to grant him an annuity calculated in some proportion to the salary he had been receiving. Sylvester was consulted as to the assessment, and characteristically insisted that account should be taken of the value to him of his government residence, medical attendance, and right of pasturage on the common.

The pension was fixed at nearly £300 per annum. He had at least one enthusiastic fellow-worker during the time he was at the establishment, Mr. (now Sir Andrew) Noble, who collaborated with him in an important degree in the papers on the Theory of Partitions.

The London Mathematical Society was founded in 1864, with De Morgan as president, a post in which Sylvester succeeded him. In June, 1865, he delivered a lecture in King's College, London, "On an Elementary Proof and Generalisation of Sir Isaac Newton's hitherto undemonstrated Rule for the Discovery of Imaginary Roots."

In 1869 he was President of the Mathematical and Physical Section of the British Association for the Advancement of Science at Exeter, the meeting being under the presidency of Stokes. His address was largely a defence of mathematics from a statement that had recently been made by Huxley in a 'Fortnightly Review' article. The latter had written, "Mathematics is that study which knows nothing of observation, nothing of experiment, nothing of induction, nothing of causation." Sylvester put in a powerful and eloquent plea for the science as one unceasingly calling forth the faculties of observation and comparison, and affording a boundless scope for the exercise of the highest efforts of imagination and invention. Those engaged in this science know the truth of Sylvester's words, but it must be admitted that men of the highest eminence in other branches of science frequently are unacquainted with the real nature of the life work of a man like Sylvester, and of that inner world of thought where the phenomena require as close attention as those which present themselves in the outer physical world. Sylvester was a philosopher, and was well able to take a survey of all the sciences. While never underrating the importance of any of the recognised divisions, he saw the intrinsic beauty of that which he loved beyond all others, and no one was more competent to repel assaults upon it, and, it may be added, no one could have been more successful in doing

so. His enthusiasm, combined with his power over the English language, made him an opponent worthy of any controversialist living. The remainder of the address was on space conceptions, and mathematics as the science of continuity. In one sentence he stated : "It is very common, not to say universal, with English writers, even such authorised ones as Whewell, Lewes, or Herbert Spencer, to refer to Kant's doctrine as affirming space to be a "form of thought, or of the understanding." This led to an interesting controversy, in the columns of 'Nature,' between G. H. Lewes, T. H. Huxley, C. M. Ingleby, G. Croom Robertson, W. H. Stanley Monck, and, of course, Sylvester himself. The correspondence, with many critical notes, will be found in an appendix to Sylvester's 'Laws of Verse' (Longmans, Green and Co., 1870).

It is doubtful if Sylvester's reputation was ever higher than at this time. The recognition of his great talents, the appreciation of his transcendent genius, and the knowledge of the inspiring effect of his personality were universal. Foreign scientific academies had showered their honours upon him. Eminent men of all countries knew him personally. A mere recital of his academic distinctions would take up too much space. It can be found in any official list of the Fellows of the Royal Society.

On leaving Woolwich Academy in 1870 he lived near the Athenæum, and for a few years his mathematical activity was in abeyance. He had some idea of becoming a candidate for the London School Board, and addressed several meetings of working-men and other assemblages of electors in London. On these occasions he would occasionally sing to contribute to the merriment of the evening. Such old English songs as "Simon the Cellarer" were his favourite pieces. He also frequently recited at penny readings. In 'The Gentleman's Magazine' for February, 1871, there appears 'The Ballad of Sir John de Courcy,' translated from the German by Syzygeticus. He recited this versified translation at the New Quebec Club and Institute at a reading on April 11, 1879.

In 1874 he entered the lists again. The occasion was the wonderful discovery by Peaucellier of the straight line link-motion associated with his name. Sylvester soon made additions and generalisations, and finally gave a Friday evening discourse on the subject at the Royal Institution of Great Britain. He showed amongst other things of great interest how to construct a link-work of seventy-eight bars to solve the following problem :—"Required to construct a link-work fixed or centred at two of its points, such that (when the machine is set in motion) some other point or points therein shall be compelled to move in the line of centres."

He wrote several papers on the subject, one of them bearing the characteristic title :—"Mode of construction and properties of a new

sort of lady's fan, and on the expression of the curves generated by any given system whatever of link-work under the form of an irreducible determinant."

He invented the plagiograph *aliter* skew pantigraph.

A synopsis only of the Royal Institution lecture was published. The manuscript of the lecture as actually delivered is in the possession of George Bruce Halstead, of the University of Texas. Extracts from it appear in the American journal 'Science,' of April 16, 1897, from which it appears that it was characterised by that eloquence, force, and poetical imagination with which students of Sylvester are familiar.

In 1875 the Johns Hopkins University, at Baltimore, was founded, and the Trustees sought the advice of the president, Daniel C. Gilman, in the selection of the professorial staff. He replied "Enlist a great mathematician and a distinguished Grecian; your problem will be solved. Such men can teach in a dwelling-house as well as in a palace. Part of the apparatus they will bring; part we will furnish. Other teachers will follow them." Joseph Henry also advised that liberal salaries should be paid and the best men in the world secured. He brought Sylvester's name prominently forward, and finally the latter was offered the post of Professor of Mathematics. He demanded a higher salary than that offered, and this being granted he finally stipulated that his travelling expenses and annual stipend of 5000 dollars should be paid in gold, and then for the second time left England to take up a professorship in the United States.

He found the conditions ideal. While not being overburdened with routine work, he was surrounded by able assistants and talented pupils only too eager to aid him in his profound original work or to catch inspiration from his lips. The mathematical staff was indeed very strong, including men of such capacity as Thomas Craig, W. E. Story, and Fabian Franklin. Sylvester's first high class consisted of but one student, G. B. Halsted. This gentleman, since well known in science, had the most beneficial effect upon his master, for it was owing to his enthusiasm and persistence that Sylvester's attention was again called to the Modern Higher Algebra and the Theory of Invariants, and a fruitful crop of new discoveries was almost the immediate result. Others, including Franklin, Durfee, Ely, and Hammond in England joined in the investigations; a school of mathematics was founded; and the American renaissance in mathematics was an accomplished fact.

Shortly after joining at Baltimore, the University founded the 'American Journal of Mathematics,' with Sylvester as editor; and its pages are evidence of the activity of the new school. In five years Sylvester himself contributed thirty papers; some of great length. They are concerned chiefly with Modern Algebra, various points in

the Theory of Numbers, the Theory of Partitions, and Universal Algebra. A splendid record for five years.

His address before the University, on Commemoration Day, February 22, 1877, was most eloquent, and had an extraordinary effect upon his hearers, amongst whom was James Russell Lowell. After some remarks concerning the work of the University and his own share therein, he discoursed upon the difficulty created by the contending claims of teacher and investigator. He said that the solution lay in the never-to-be-forgotten words, which had recently been addressed to him, "The University desires from you your best and highest work." He went on to observe on the religious and other disabilities under which students in English universities had suffered, and brought into contrast the freedom in American and German universities. He spoke with as much warmth as power for, as he said, the subject came home to him. For some time he held his audience spell-bound. His speech was never finer than when under the influence of passion, and he abandoned himself to a torrent of words. Those competent to form an opinion believe that there was within him the material of a great orator.

The following remarks by some of his pupils in Baltimore are of interest as showing his character and method of lecturing.

Dr. E. W. Davis states :—

"Sylvester's methods! he had none. 'Three lectures will be delivered on a New Universal Algebra,' he would say; then 'the course must be extended to twelve.' It did last all the rest of that year. The following year the course was to be 'Substitution Theory, by Netto.' We all got the text. He lectured about three times, following the text closely, but stopping sharp at the end of the hour. Then he began to think about Matrices again. 'I must give one lecture a week on these,' he said. He could not confine himself to the hour nor to the one lecture a week. Two weeks passed and Netto was forgotten entirely and never mentioned again."

Mr. A. S. Hathaway says :—

"I can see him now, with his white beard and few locks of grey hair, his forehead wrinkled o'er with thoughts, writing rapidly his figures and formulæ on the board, sometimes explaining as he wrote while we, his listeners, caught the reflected sounds from the board. But stop, something is not right; he pauses, his hand goes to his forehead to help his thought; he goes over the work again, emphasizes the leading points, and finally discovers his difficulty. Perhaps it is some error in his figures, perhaps an oversight in the reasoning. Sometimes, however, the difficulty is not elucidated, and then there is not much to the rest of the lecture. But at the next lecture we would hear of some new discovery that was the outcome of that difficulty, and of some article for the journal that he had

begun. If a text-book had been taken up at the beginning, with the intention of following it, that text-book was most likely doomed to oblivion for the rest of the term, or until the class had been made listeners to every new thought and principle that had sprung from the laboratory of his mind, in consequence of that first difficulty. Other difficulties would soon appear, so that no text-book could last more than half the term. In this way the class listened to almost all of the work that subsequently appeared in the journal. It seemed to be the quality of his mind that he must adhere to one subject. He would think about it, talk about it to his class, and finally write about it for the journal. The merest accident might start him, but, once started, every moment, every thought was given to it, and, as much as possible, he read what others had done in the same direction; but this last seemed to be his weak point; he could not read without meeting difficulties in the way of understanding the author. Thus, often his own work reproduced what others had done, and he did not find it out until too late."

Dr. W. P. Durfee, Professor of Mathematics at Hobart College, Geneva, N.Y., has written:—

"His manner of lecturing was highly rhetorical and elocutionary. When about to enunciate an important or remarkable statement he would draw himself up till he stood on the very tips of his toes, and in deep tones thunder out his sentences. He preached at us at such times, and not infrequently he wound up by quoting a few lines of poetry to impress upon us the importance of what he had been declaring."

On the death of H. J. S. Smith, Sylvester was elected to the Savilian Professorship of Geometry at Oxford, and in December, 1883, he finally left Baltimore to enter upon residence in New College, Oxford. At the time his mind was occupied with the theory of a new species of invariants; these are differential and of more immediate application to geometry than those of pure algebra. Sophus Lie had treated the whole subject of differential invariants from a general point of view, and had given the various categories, but had made no attempt to develop the special case treated by Sylvester. His notice appears to have been first attracted to the subject by the well-known invariantive property of the differential expression known as the Schwarzian Derivative, which in this country had been studied by Cayley and Forsyth. The invariantive forms, he quickly reached, he termed reciprocants, the name arising from the fact that, from his original point of view, the expressions arrived at were unchanged, to a *factor près*, by the simple interchange of the dependent and independent variables. Later he considered the general linear and homographic transformations applied to similar forms, and propounded an extensive theory of great geometrical

importance. The lectures on the theory were delivered before the University of Oxford during the Hilary, Easter, and Michaelmas terms of 1886, and subsequently published in the 'American Journal of Mathematics.' The powerful weapon chiefly employed in the research is due to the author himself, and is termed by him "the method of infinitesimal variation." In many details, and in the orderly exposition, he was greatly assisted by James Hammond, M.A., and his fellow-workers in Oxford—E. B. Elliott, C. Landesdorf, and L. J. Rogers—and others outside the University also made notable contributions. In particular, L. J. Rogers made a capital discovery in the Theory of Principiants (the name given to those reciprocants which are invariantive in respect of the homographic substitutions), which gave Sylvester material for most of the lectures in the latter half of the series.

This theory was Sylvester's last great work. A masterly contraction of Tchebicheff's limits with regard to the number of primes occurring between given numbers, and a tract upon Buffon's problem of the needle, are the only other papers that need mention.

Failing health, frequently involving acute suffering, came upon him when he was close upon eighty years of age. His high sense of the duties appertaining to his position would not allow him longer to attempt actively to lead the mathematical studies of the University, and in 1893 a deputy professor (Mr. W. Esson, F.R.S.) was appointed.

The remaining three years witnessed the gradual breaking up of an iron constitution. He lived for the most part with friends, or in apartments in or near Mayfair, with occasional visits to Tunbridge Wells, where he stayed at the Spa Hotel. For some years he was quite unable to think of mathematical subjects. He found that he could no longer understand notes that he had made in former years, and this made him sad and dejected. About August, 1896, a revival of energy and mental power took place, and till his death, March 15, 1897, he worked continuously at the Theory of Compound Partition, and made an heroic attempt to prove or disprove the celebrated Goldbach-Euler conjecture concerning the partition of every even number into two primes. A fortnight before his death, while working in his sitting-room at Hertford Street, Mayfair, he dropped his pen, and on stooping to pick it up had a paralytic seizure. He never spoke again, and continuously sank until the end came.

He was a Royal Medallist of 1861, and the Copley Medallist of 1880.

While it is certain that he was one of the greatest mathematicians of all time, it may be doubted whether he will take a place amongst the small band who occupy absolutely the front line. His character and temperament militated against continuity of thought. He would

be oppressed with a flood of ideas, which made it difficult for him to suitably organise his researches. A theory, half composed, would be forsaken that he might grapple with fresh imaginations. It is certain that but a small fraction of his best work has been published for the benefit of posterity. His genius and his greatness are not properly represented by the memoirs which he has left. He had, to some extent, the poetic faculty, and occasionally occupied himself with the composition of sonnets, both in English and Latin.

He had literary power, and considerable knowledge of languages, living and dead. He wrote French with ease, and conversed readily in French, German, and Italian. He was acquainted with both Latin and Greek, and when past seventy-five years of age read 'Athenæus' without a dictionary.

The writer, who had numerous opportunities of studying the character of this illustrious man in the last years of his life, when he was heroically battling against acute suffering, consequent upon the infirmities of extreme old age, formed the conviction, that will never be shaken, that his personal character was one of singular beauty, and that its salient points were simplicity and honesty. Absolutely and fearlessly honest from cradle to grave. Future generations will mark with admiration the deep footprints he has left upon the sands of time, but they will not be able to realise the effect which contact with his great spirit had upon his contemporaries, who knew and loved him. The superficial crust of eccentricities and slight faults of temperament once pierced, and the kernel of his nature reached, there was found a roundness and perfection of disposition that is not often met with.

It cannot, perhaps, be said that his religious convictions were of a kind which could be completely defined, but it is certain that he believed in a Supreme Being, and in a future life, a life full of enhanced intellectual power, and opportunities of intellectual growth. The atheist will find nothing to give him satisfaction in the story of this life, throughout which the faith was strong, and the conviction that high principle should be paramount always present.

His last sufferings, extending over fifteen days, were borne with fortitude.

So passed away one of the great spirits of the century.

"And he is gathered to the Kings of Thought."

P. A. M.

ALFRED LOUIS OLIVIER LE GRAND DES CLOIZEAUX, who died on May 6, 1897, in the eightieth year of his age, was born at Beauvais, Département de l'Oise, on October 17, 1817, and belonged to an old magisterial family. He was educated at Paris, and the teacher to

whom he went for instruction in "special mathematics" was the crystallographer Lévy: by him the young Des Cloizeaux was initiated into the mysteries of that science, and was advised to enter upon the mineralogical course then being given by Dufrenoy, at the École des Mines. At that institution he later made the acquaintance of Sénarmont, through whose influence it was that he afterwards came to devote himself so closely to the optical investigation of crystals. His first paper dealt with the crystallisation of *Æschynite*, and appeared in 1842; for the next half-century scarcely a year passed without the issue of one or more papers recording the results of some mineralogical research. In 1843 he was appointed Répétiteur at the École Centrale, and in 1857 Maître des Conférences at the École Normale; from 1873 to 1876 he took the place of Delafosse at the Sorbonne, and, on the retirement of that professor from his office at the Paris Natural History Museum, Des Cloizeaux was appointed (1876) to the Curatorship of that important mineral collection. This office was particularly congenial to his tastes, for he was always more happy in the laboratory than in the lecture-room; he retained it till 1892, when the rules of the Civil Service necessitated his retirement at the age of seventy-five.

Though his life-work related almost wholly to the morphology and optics of crystals, his inquiries were not limited to the examination of museum specimens; he was much interested in modes of origin and of occurrence, and in the geological relations of minerals; and he was always ready to seek an opportunity for seeing in its native home any mineral to the determination of the characters of which he had been devoting his attention. He went on two missions to Iceland (1845-6) to investigate the mode of occurrence of the well-known spar, about the scarcity of which physicists had already become anxious, and he brought back from that island many specimens of its minerals and rocks useful to him in his later researches; in 1868 he was sent on another special mission, on this occasion to Norway, Sweden, and Russia; and in the course of a long life he found it practicable to visit all the more important mineral localities of the Continent.

Two characteristics specially manifest in the work of Professor Des Cloizeaux are accuracy and perseverance: he would spare no pains to obtain the best values for the crystallographic constants of the material he was investigating, while, a problem once started upon, he would persist in its investigation for years, and return to it again and again as new specimens or new methods became available. For the first twelve or thirteen years his work was almost wholly morphological, and consisted in the determination of the crystallographic constants of rare or new specimens and in the description of the minerals associated together at new localities. To this epoch belongs his memoir on the crystallisation of quartz; to the thirty-

five then known forms of this common mineral he added no fewer than 135 new ones, many of them with complicated indices determined through the skilful use of zones: as to the remarkable stereographic projection prepared by Des Cloizeaux to illustrate the observed forms of quartz, Professor Ruskin, speaking as an artist and critic, has more than once expressed to the present writer his great admiration of the "patient labour and entire accuracy of workmanship" therein displayed.

From 1855 onwards his researches were mainly optical. It must be remembered that at the time when M. Des Cloizeaux entered upon the study of minerals, the instrumental appliances for the optical examination of small crystals were in the rudimentary stage of development, and were scarcely in use outside physical laboratories. He improved the polarising microscope of Nörremberg, giving it a new form, increasing the field of view, and making it a convenient instrument for the examination of small sections. Thereupon, not only did he entertain the colossal idea of determining the optical characters of all known crystals, whether natural or artificial, but he began the work. He was soon led to emphasise the importance of the "optical sign" for the discrimination of minerals, and in his determination of the crystallographic system to make constant use of the peculiarities of the distribution of colour in the rings afforded by the sections of crystals in convergent polarised light. In 1868 he published his observations of the changes of optic axial angle resulting from changes of temperature. In the case of orthoclase he found that, with a maximum temperature of 400° C. only temporary changes were induced, but that with a temperature exceeding $700\text{--}800^{\circ}$ permanent changes resulted. Further, he pointed out that in the case of orthoclase from some volcanic rocks this changed optical condition was already a character of the specimens.

He was the discoverer of the circular polarisation of cinnabar, and showed that it was seventeen times that of quartz. He was the first to find a substance (strychnine sulphate) which rotates the plane of polarisation, both in the crystallised state and in solution. He showed that benzil circularly polarises when in crystals, but is inactive when fused or in solution, and that, on the other hand, camphors are active only when in solution. He was the first to show, by optical characters, that there are ortho-rhombic members of the epidote, pyroxene, and amphibole groups, and that the three types of humite have characteristic optical features.

But more especially was he interested in the feldspars, a group of minerals of fundamental importance in the classification of rocks, and to the investigation of which he gave more than twenty years of his life. When he began this work it seemed unlikely that much was left to be discovered in the case of so long known a group, and

it was a veritable triumph for his method of work that he was able to establish that there was a kind of potash-felspar distinct from orthoclase; it was anorthic instead of mono-symmetric in its symmetry, although approximating to the latter in the development of its forms, and its optical characters, instead of being unstable, are stable at all temperatures; to this he applied the term microcline. Indeed, it is to Des Cloizeaux that we owe our first precise knowledge of the optical characters of all the plagioclastic felspars, and the determination presented constant difficulty to him by reason of the lowness of the symmetry and the smallness and rarity of well-developed crystals.

Much of his work has been incorporated in the treatise to which he gave the modest title '*Manuel de Minéralogie*,' and which is now the standard book of reference for all that relates to the optics of minerals. In its compactness and freedom from unnecessary words, and indeed in its general characters, it bears a close resemblance to (Brooke and) Miller's edition of '*Phillips's Mineralogy*'; and it is interesting to know that at first he had intended merely to translate that book, but was eventually compelled, by the extension of his optical researches, to prepare an independent treatise. He adopted the same general plan in giving a stereographic projection of the observed faces for all the more important minerals, and elaborate lists of measured and calculated angles useful in the recognition of the substance. The preparation of this manual was a work of great labour, and involved a vast amount of physical observation and numerical calculation; he made it a rule never to cite an angle without verification by observation or recalculation. The first volume of the manual was published in 1862; the first part of the second volume in 1874, and the second part in 1893. The third and last volume had not been issued at the time of his death, but is now being prepared for publication by his successor at the Museum and former pupil, Professor Lacroix.

Professor Des Cloizeaux was elected Membre de l'Institut in 1869, and President of the Académie des Sciences in 1889. The Royal Society awarded him the Rumford Medal in 1870, and elected him a Foreign Member in 1875. He was a founder and the first President (1878) of the Mineralogical Society of France, and again served as its President eleven years later. Other Societies in many parts of the world recognised the value of his scientific work by enrolling him on the list of Honorary Members. An aged widow, a widowed daughter (the Vicomtesse d'Hérouville), and three young grandchildren more especially mourn his loss; but the memory of his kindly character and encouragement will long be treasured up by those who were in any way associated with him.

L. F.

By the death of JOHN CARRICK MOORE, science loses the last of that band of ardent field-geologists who, during the first half of the present century, did so much to investigate the underground structure of the British Islands. Inspired by the example and animated by the scientific principles of William Smith, they carried out in fuller detail than was possible to their master, his great idea of delineating in maps and sections the distribution and relations of the British strata—guided everywhere by the organic remains which they contain. But while this band of workers—which included such names as those of Buckland, Conybeare, Webster, Mantell, Dixon, Lonsdale, Sedgwick, Murchison, Fitton, De-la-Bèche, Godwin-Austen, and Phillips—were so deeply influenced by the teaching of William Smith, yet they were seldom, with the exception of the last-mentioned, personally instructed by him, but derived their knowledge of his principles and methods at second hand from men like Richardson, Townsend, and Farey, who were proud to act as the disciples and interpreters of the distinguished “Father of English Geology.”

John Carrick Moore came of a very famous stock. His grandfather, Dr. John Moore, the friend and biographer of Smollett, was the author of many works very famous in his day, of which the novel “Zeluco” has been longest remembered. Three of the sons of Dr. John Moore had very distinguished careers. The eldest surviving son was General Sir John Moore, the hero of Corunna, and a younger son was Admiral Sir Graham Moore, whose exploits on the sea were scarcely less notable than those of his elder brother in the field. The father of John Carrick Moore was James Moore, the second surviving son of Dr. John Moore, who studied medicine in Edinburgh and London, and became one of the most distinguished surgeons of his day. He was the friend of Jenner, and, as a well-known writer in favour of vaccination, was appointed to succeed that surgeon as director of vaccine establishments.

James Moore, who practised extensively for many years in London, was the author of many medical treatises and of a biography of his brother, General Sir John Moore, published in 1833. Having had bequeathed to him by a Mr. Carrick, a banker in Glasgow, the estate of Corsewall, in Wigtownshire, near Stanraer and Port Patrick, James Moore added to his own surname that of Carrick. In 1825 James Carrick Moore retired from practice, and, having built himself an excellent house upon his estate on the shores of Loch Ryan, spent the remainder of his life there, dying in 1834 at the age of 71. On their mother's side, the Moores were descended from Robert Simson, the celebrated geometrician.

John Carrick Moore was the second son of James Carrick Moore, and was born in 1804. He went to Cambridge, and was educated at Queen's College, proceeding to the degree of M.A., and devoting

much attention to mathematics and physics. Before the year 1838, his attention seems to have been attracted by the rocks of the Rhinns of Wigtownshire, near his residence, for we find that he was in communication with Charles Lyell, who identified the fossils found by him as graptolites. In the year named, he was elected a fellow of the Geological Society.

In 1839 he traced out carefully the succession of strata along the west shore of Loch Ryan, and in the following year a paper on the subject was read by him to the Geological Society. In 1841, Sedgwick, crossing from Ireland, paid a visit to Corsewall, and was accompanied by John Carrick Moore in a tour through Ayrshire. In September 1843, Lyell and his wife paid a visit to the same hospitable dwelling, examining and confirming the accuracy of Moore's sections. Much of Lyell's time seems to have been spent in studying the rain- and hail-prints, with the fucoid- and crustacean-markings on the shores of Loch Ryan, and he subsequently wrote to Moore: "The Loch is a grand magazine of geological analogies—tidal, littoral, conchological, sedimentary, &c., which I envy you having at your door." Subsequently to this visit, Lyell, under the direction of Moore, visited the remarkable rocks in the neighbourhood of Ballantrae and bore testimony to the accuracy of his friend's work there.

In 1846 we find John Carrick Moore had become so identified with the work of the Geological Society, that he was elected Secretary, and in the same year he became a member of the Geological Society Club. He held the office of Secretary for six years (1846–52), when he was elected a Vice-President of the Society (1853–4), resuming his post of Secretary in 1855 for one year. So active indeed was Carrick Moore in the administration of the Geological Society's affairs, that between 1846 and 1875 we find him absent from the Council only in four years; he was a Vice-President in 1862, and again in 1864–5. In 1848 he read a more extended paper to the Geological Society on the Silurian rocks of the Wigtownshire coast, the fossils being described and figured by Salter. In 1856 and 1858 Moore communicated accounts of further observations on Wigtownshire geology to the Geological Society, while his general interest in geological research was shown by the papers written by him in 1850 and in 1863, on fossils collected and sent home from San Domingo by Mr. Heniker, and from Jamaica by Lucas Barrett. In 1849 we find him describing the Oligocene fossils found in the New Forest.

John Carrick Moore was proposed as a Fellow of the Royal Society in November, 1855, his nomination paper being signed first by his friend Charles Lyell, while others who subscribed from personal knowledge were Sedgwick, Murchison, Hopkins, Leonard

Horner, and Faraday. He does not appear, however, to have ever contributed a paper to the Society. By his patient labours in studying the geology of Galloway he made valuable additions to our knowledge of the stratified rocks of Britain, and he took a distinguished place among the band of amateur workers—including many landed proprietors, clergymen, soldiers, and doctors—to whose painstaking and detailed work in the field English geology owes so much. Among these men, John Carrick Moore was always held in the highest esteem, and his time and energy were ungrudgingly devoted alike to the advancement of his favourite science by careful studies in the field, and to the promotion of the interests of the Society identified with that science, during the parts of the year when he resided in London.

In 1864, Andrew Ramsay spent a few days with John Carrick Moore at Corsewall, mapping the peninsula, which terminates in Corsewall Point, for the Geological Survey of Scotland. Of John Carrick Moore's wide sympathies with all matters connected with geology, and of the knowledge and ability with which, owing to his early training at Cambridge, he was able to deal with those questions of physical geology demanding an acquaintance with mathematical methods, we have abundant evidence. Between 1865 and 1867, he sent a series of letters to the 'Philosophical Magazine,' dealing in a very able and critical manner with Ramsay's theory of the origin of lake-basins, and with Croll's theory of the cause of the glacial period. These letters show that Moore had not forgotten his early training and had kept himself abreast of the science of the day by his studies of physical questions; and the substantial justice of his criticisms has been abundantly shown by later researches. In 1875 he wrote to 'Nature,' pointing out a curious oversight of Humboldt in his 'Cosmos.'

In 1875, John Carrick Moore finally withdrew from the Council of the Geological Society, upon which he had served so long and so faithfully; and from that time forward he would seem to have ceased to take any active part in scientific work. Few of the present generation of geologists can even recollect having seen the stately and courteous gentleman, who was at one time so indefatigable in the service of their society, and who had so frequently acted as one of its officials. For nearly a quarter of a century after this withdrawal from public activity, however, John Carrick Moore lived on, spending his time between his seat in Wigtownshire and the house in Eaton Square, where he died on February 10, 1898, at the great age of 94. His only son had pre-deceased him, but a daughter survives, the estate passing to his nephew Colonel Sir David Carrick Buchanan, of Drumpellier. Besides the Corsewall estate, John Carrick Moore owned property in Kirkcudbrightshire and in England, and he was

a deputy-lieutenant of the county of Wigtownshire. He was not less highly respected among the gentry of his county and the tenants of his estate than in the circles of scientific society in London, in which his presence was so long conspicuous.

J. W. J.

By the death of Baron FERDINAND VON MUELLER Australia has lost a botanist and geographer who stood in the foremost rank of the scientific men of the southern hemisphere. Ferdinand Jakob Heinrich Mueller was born in 1825 at Rostock, of which town his father was Commissioner of Customs. He was educated for the medical profession at Kiel, where he graduated as Ph.D., after having devoted much of his time as a student to the botany of Schleswig and Holstein. Soon after attaining his majority he was seized with an affection of the lungs, and having lost both parents from consumption, he resolved to seek a more genial climate than that of North Germany. He accordingly in 1847 left for Australia, to which country there was then a considerable emigration from Germany. This was not till after the completion of his first botanical essay, "*Breviarium Plantarum Ducatus Slesvicensis austro-occidentalis*," which was not published till 1853.* He had meanwhile been enrolled as a member of the German Association for the Advancement of Science, which had just been instituted by Oken.

After his arrival in Australia, Mueller acted for a short time as assistant to a chemist in Adelaide, but being at once fascinated by the interest and novelty of the flora, and having apparently some private means, he gave himself up unreservedly to botanical and geographical exploration. Leaving Adelaide, he crossed over to Victoria in 1848, with the especial object of visiting the then all but unknown Australian Alps, and connecting their flora with that of Tasmania.

During the several years devoted to this object, he, alone and unaided except by the contributions of a few generous friends, displayed great intrepidity as an explorer, penetrating into the interior as far as the Murray River, in crossing which he nearly lost his life, effecting the first triangulation of any part of the Victorian Alps, and making extensive botanical collections abounding in novelty and interest. At the same time he entered into correspondence with botanists in Europe, sending them duplicates of his discoveries, and letters that at once established his reputation as a young naturalist of great attainments and astonishing powers of work.

Amongst his English correspondents was Sir W. Hooker, who interested himself in his favour with Mr. Goulburn, then on the point of leaving England as Lieutenant-Governor of Victoria, and who

* '*Flora*,' vol. 36, p. 473.

was desirous of having the vegetable resources of that Colony turned to the best account. This resulted in the creation of a Department of Botany in the public service of Victoria, and the appointment of Mueller to its directorship.

In July, 1855, an expedition was organised at Sydney, with the view of discovering the fate of Leichardt, who in 1847 had started with a fully-equipped party in an endeavour to cross the continent of Australia from east to west, but of whom no tidings had been obtained for seven years. The search party was conducted by Mr. A. C. (afterwards Sir Augustus) Gregory, and Mueller was attached to it as botanist. Leaving Sydney in July of that year in the barque "Monarch," the expedition sailed round the north coast of Australia to the Victoria River, on the north-west coast of the continent, and after spending a year in the exploration of the sources of that river to the 17th degree of S. latitude, it returned by land across the continent, skirting the Gulf of Carpentaria, and finally reaching the Darwin River in Queensland, in November, 1856. During this remarkable journey nearly 20° of unexplored country were traversed, and Mueller, who proved himself an invaluable member of the expedition, obtained magnificent collections of plants abounding in novelties, all carefully annotated and in perfect condition. He subsequently made two other extended land journeys, both in Western Australia, one in 1867, when he explored the country between King George's Sound and the Stirling Range; the other to the district east of Shark's Bay, between the Murchison and Gascoigne Rivers.

In 1857 Mueller was appointed, at a suitable salary, Director of the Melbourne Botanical Gardens, with herbarium, library and laboratory, which post he held till 1873, when he was deprived of the administration of the gardens on the ground of his paying too much attention to the introduction and cultivation of plants of purely scientific interest, and too little to the æsthetic requirements of the Melbourne public, who desired to see their extensive public grounds and garden rival in beauty the far-famed and no less scientific establishment of the same kind at Adelaide in the adjoining colony of South Australia. The fact is, that, great as were Mueller's contributions in many ways to horticulture and gardening in Australia and Europe, he was neither a practical horticulturist nor a landscape gardener. On the occasion of his demission the Colonial Government treated him with great consideration and liberality, retaining his services as Government botanist, with residence, undiminished salary, herbarium, library, and laboratory.

The principal labours of Mueller may be classed under the two heads of scientific and economic botany, especially forestry. It is impos-

sible here to give even a list of his scientific publications. Besides 104 papers registered in this Society's 'Catalogue of Scientific Papers,' he produced many works of exceptional value. Amongst them the most notable are the 'Fragmenta Phytographiæ Australiæ,' begun in 1858 and concluded in 1882, comprised in twelve volumes, a work teeming with critical observations on Australian plants which have been embodied by Bentham in the 'Flora Australiensis'; the 'Eucalyptographia,' a revision of the Gum-trees of Australia, with 129 illustrative plates; the 'Iconography of Australian Salsolaceous Plants,' with 90 plates; the 'Acaciæ and their Allies,' with 130 plates; the 'Myoporinæ,' with 74; and the 'Plants of Victoria,' a fragment, with 90. The descriptive portions of these works leave nothing to be desired from a scientific point of view, and the plates, all in quarto, abounding in anatomical analyses, and executed altogether in the Colony, rival the best of those of European botanical works. In 1882 he published his 'Census of Australian Plants,' in which the ranges of the species in the several Colonies are given, thus initiating a botanical geography of the continent. A second edition appeared in 1889.

From his first years in Australia, Mueller had entertained the ambition of writing a Flora of that continent, and when the several Colonial Governments acceded to the representations of Sir W. Hooker that such a work should be undertaken, and had voted the supplies for its execution, the name of Mueller was naturally the first to be suggested as author. And he no doubt would have been selected but for the fact that without constant access to the Australian collections in the British Museum and at Kew it could not be accomplished. Mueller at once grasped the situation, and, hearing that Bentham had been selected as author, he generously offered the use of the whole of his materials, including that of his immense herbarium, which he transmitted, by instalments, to Kew for the purpose. This great work, commenced in 1863, was concluded in 1878, Mueller loyally aiding by correspondence from beginning to end. Happily his collections were returned to him without the loss of a specimen.

Of Mueller's works in economic botany the most important is the 'Select Extra-tropical Plants, suitable for Industrial Culture or Naturalisation in Australia.' This work is remarkable as a monument of botanical erudition, and, as an economic guide, it is unique of its kind. It passed through many editions in the colony, has been translated into four European languages, and been reprinted in the United States and in India. Besides being the means of introducing many new cultures into Australia, Mueller's activity in sending seeds of Australian plants, especially trees, all over the world, was phenomenal; and to him South Europe, Algeria, India, South and West Africa, California, and South America, are greatly indebted for the groves of

eucalypti, acacias and other trees that have done so much to adorn their hills and plains, and even to improve their climates. To the Royal Gardens, Museums, and Herbarium of Kew he was a perennial contributor of botanical treasures, continuously for upwards of forty years, often at considerable personal cost. Of this the magnificent specimen of the great fern, *Todea barbara*, in the Temperate House, is a conspicuous example. It is a native of gullies in the Victorian Alps, from whence Mueller had it transported by wagon to Melbourne, at his own expense, and shipped from thence, as a gift, to Kew.

After botany the furtherance of geography was Mueller's constant endeavour. He was President of the Geographical Society of Victoria from its commencement, and author of valuable contributions to its Proceedings. According to a statement in the 'Melbourne Argus,' it was he who induced Sir W. Macgregor to undertake the exploration of New Guinea. He was an active member of Burke and Will's Exploration Committee, and he ceaselessly urged upon the attention of his fellow-colonists the importance of an Antarctic Expedition. No better evidence could be adduced as to the value attached to his own explorations and his efforts in the advancement of geographical knowledge, than that at the Geographical Congress in Vienna he was one of the first to whom a special vote of thanks was awarded for exceptional services in the cause of this science.

Amongst other instances of his devotion to science must be recorded the fact that he was one of the three founders of the now flourishing Royal Society of Victoria, which was established within a year of his arrival in the colony. He was President of the Australian Association for the Advancement of Science at its second session, held in Melbourne in 1889, and was an active member of the Horticultural, Acclimatisation, and various other societies of the Colony. It may further be mentioned, that being a discriminating devotee of music, he was chosen acting President of the Melbourne Liedertafel.

As with many other men of ardent disposition, Baron Mueller had striking personalities. He is described as being of middle height and frugal habits, dressing in black, wearing wooden shoes, and boasting of never having been possessed of a watch or a looking-glass. He was as voluble in conversation as indefatigable in correspondence, asserting that the latter amounted to 3000 letters annually, written with his own hand. His multitudinous titles, and the decorations with which he delighted to adorn himself, were a source of innocent gratification to him, especially his foreign hereditary dignity of Baron, conferred on him by the King of Wurtemberg; and the K.C.M.G. by Her Majesty on the announcement to the Secretary of State for the Colonies of the completion of the

'Flora Australiensis.' He was generous to a fault, devoting the whole of the savings from his official salary to science, charities, and good works. He was elected a fellow of the Linnean Society in 1859, of the Royal in 1861, and was awarded a Royal Medal in 1888. Of other British and foreign scientific societies he held 150 diplomas. He never married. His last and fatal illness was an affection of the brain, of a fortnight's duration, due to study, worry, insomnia, and a total abandonment of bodily exercise. He died in his official residence in Melbourne, October 9th, 1896.

J. D. H.

INDEX TO VOL. LXIII.

- Air, Gases in (Ramsay and Travers), 437; new Constituent of (R. and T.), 405.**
Aleurone-layer, Function of, in Germination (Brown and Escombe), 3.
Alternate Currents, Aluminium-Carbon Cell as "Rectifier" of (Wilson), 329.
Aluminium Anode, apparent Great Resistance of Film on (Wilson), 329.
Amagat (Professor) admitted, 373.
Amnion, a Post-amniotic Canal in Sphenodon (Dendy), 440.
Animal Heat Calorimetry; Relation to Oxygen Absorbed; Effect of Food (Marcet and Floris), 242.
Argon, Companions of (Ramsay and Travers), 437; Position among Elements (Crookes), 408.
Ashworth (J. H.) The Stomodæum, Mesenterial Filaments, and Endoderm of *Xenia*, 443.
Azores, Importance of Meteorological Observatories in (Prince of Monaco), 206.
- Baker (Henry Frederick) elected, 373; admitted, 412.**
Bakerian Lecture (Russell), 102.
Balances, Experimental Investigations on the Oscillations of (Mendeléeff), 454.
Barometers, Aneroid, Experiments on (Chree), 401.
Basaltic Rocks, Magnetic Susceptibility of (Rücker and White), 460.
Batten (F. E.) Experimental Observations on the Early Degenerative Changes in the Sensory End Organs of Muscles, 61.
Blackman (Vernon H.) On the Cytological Features of Fertilisation and related Phenomena in *Pinus silvestris*, L., 400; — and Murray (G.) A Study of the Phyto-Plankton of the Atlantic, 269.
Bose (Jagadis Chunder) On the Production of a "Dark Cross" in the Field of Electromagnetic Radiation, 152; on the Rotation of Plane of Polarisation of Electric Waves by a Twisted Structure, 146.
Brown (Ernest William) elected, 373.
Brown (Horace T.) and Escombe (F.) On the Depletion of the Endosperm of *Hordeum vulgare* during Germination, 3.
Buchan (Alexander) elected, 373.
Burch (George J.) On Artificial Temporary Colour-blindness, with an Examination of the Colour Sensations of 109 Persons, 35; — and Gotch (F.) The Electrical Response of Nerve to a single Stimulus investigated with the Capillary Electrometer. Preliminary Communication, 300.
- Calorimeter for the Human Body (Marcet), 232.**
Candidates, List of, 1; recommended for Election, 212.
Capillary Electrometer, Electrical Changes in Nerve recorded by (Gotch and Burch), 300.
Capstick (J. W.) On the Kathode Fall of Potential in Gases, 356.
- VOL. LXIII.

- Cherry (T.) and Martin (C. J.) The Nature of the Antagonism between Toxins and Antitoxins, 420.
- Chlorophyll and Derivatives, Absorption Spectra of (Schunck), 389.
- Chree (C.) Experiments on Aneroid Barometers at Kew Observatory and their Discussion, 401.
- Colour-blindness, Artificial Temporary (Burch), 35.
- Combination Tones, Objective Reality of (Forsyth and Sowter), 396.
- Contact Electricity of Metals affected by State of Surfaces, by Films, Exposure and Temperature (Erskine-Murray), 113.
- Crookes (Sir William) On the Position of Helium, Argon, and Krypton in the Scheme of Elements, 408.
- Croonian Lecture (Pfeffer), 93.
- Crystallisation, Modifications by Mud (Raisin), 217.
- Crystals of Cubic System with Cubic Cleavage, structure of (Sollas), 270, 286, 296.
- Darwin (Francis) Observations on Stomata, 413.
- Dendy (Arthur) Summary of the Principal Results obtained in a Study of the Development of the Tuatara (*Sphenodon punctatum*), 440.
- Des Cloizeaux (A. L. O. Le G.) Obituary Notice of, xxv.
- Dewar (James) Makes Preliminary Communication on Liquefaction of Hydrogen and Helium, 231; Preliminary Note on the Liquefaction of Hydrogen and Helium, 256; — and Fleming (J. A.) On the Magnetic Susceptibility of Liquid Oxygen, 311.
- Dilatometer, Compensated Interference (Tutton), 208.
- Diphtheria—Chemical Nature of Antagonism of Toxins and Antitoxins (Martin and Cherry), 420.
- Dobbie (J. J.) and Gray (Andrew) On the Connection between the Electrical Properties and the Chemical Composition of different kinds of Glass, 38.
- Echinoid Larvæ, relations between Hybrid and Parent Forms of (Vernon), 228.
- Eclipse, Solar, Preliminary Communications made, 205.
- Edser (Edwin) An Extension of Maxwell's Electro-magnetic Theory of Light to include Dispersion, Metallic Reflection, and allied Phenomena, 91, 374.
- Election of Fellows, 373.
- Electrical Discharge in Rarefied Gases; Circumstances causing Intermittence (Capstick), 356.
- Electrodynamic Equations in Moving Media (Larmor), 365.
- Electro-magnetic Radiation, production of a "Dark Cross" in Field of (Bose), 152.
- Electro-magnetic Waves, Polarisation and Rotation of Polarisation Plane (Bose), 146.
- Electrostriction (Larmor), 365.
- Elements, Scheme of—Position of Argon, Helium, and Krypton (Crookes), 408.
- Ellis (William) On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force and the Period of Solar Spot Frequency, 64.
- Enamel of Elasmobranch Fishes, Structure and Development of (Tomes), 54.
- Endoderm in *Xenia*, long Pseudopodia of (Ashworth), 443.
- Endosperm, Relation of, to Embryo, Depletion, Vitality (Brown and Escombe), 3.
- Erskine-Murray (J.) On Contact Electricity of Metals, 113.

Escombe (J.) and Brown (Horace T.) On the Depletion of the Endosperm of *Hordeum vulgare* during Germination, 3.
 Evolution, Mathematical Contributions to the Theory of. V. (Pearson), 417.

Falmouth Magnetic Observatory. Note concerning Dip Observations, 480.

Farmer (J. B.) and Waller (A. D.) Observations on the Action of Anæsthetics on Vegetable and Animal Protoplasm, 213.

Fellows elected, 373.

Fern Prothalli, Apogamy and Development of Sporangia upon (Lang), 56.

FitzGerald (Geo. Fras.) Note on the Connection between the Faraday Rotation of Plane of Polarisation and the Zeeman Change of Frequency of Light Vibrations in a Magnetic Field, 31.

Fleming (J. A.) and Dewar (James) On the Magnetic Susceptibility of Liquid Oxygen, 311.

Flicker, and Duration of Undiminished Impression on Retina, Fundamental Laws of (Porter), 347.

Floris (R. B.) and Marcet (W.) An Experimental Enquiry into the Heat given out by the Human Body, 242.

Forayth (R. W.) and Sowter (R. J.) On Photographic Evidence of the Objective Reality of Combination Tones, 396.

Functions, Algebraic, Connection with Automorphic (Whittaker), 267.

Germination of *Hordeum vulgare*, Depletion of Endosperm (Cytohydrolysis and Starch-erosion) during (Brown and Escombe), 3.

Glass, Electrical Properties (Resistance and Specific Inductive Capacity) as related to Chemical Composition (Gray and Dobbie), 38.

Gotch (F.) and Burch (G. J.) The Electrical Response of Nerve to a Single Stimulus investigated with the Capillary Electrometer. Preliminary Communication, 300.

Gray (Andrew) and Dobbie (J. J.) On the Connection between the Electrical Properties and the Chemical Composition of different kinds of Glass, 38.

Haloid Salts of Alkalis and Silver, Structure of (Sollas), 270, 286.

Harmer (Sidney Frederic) elected, 373; admitted, 412.

Haswell (W.) admitted, 373.

Heat, dissipated by Platinum Surface (Petavel), 403; given out by Human Body, Experimental Enquiry into (Marcet and Floris), 242.

Helium, Liquefaction of (Dewar), 256; Position among Elements (Crookes), 408.

Hybrid and Parent Forms of Echinoid Larvæ (Vernon), 228.

Hydrogen, Liquefaction of (Dewar), 256.

Hysteresis and Permeability, Effect of Temperature on (Roget), 258.

Induction, Mutual, of a Circle and a Coaxial Helix (Jones), 192.

Iodide of Silver, Contraction with Rise of Temperature (Sollas), 286.

Jones (E. Taylor) On the Magnetic Deformation of Nickel, 44.

Jones (J. Viriamu) On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helical Current and a uniform coaxial circular cylindrical Current Sheet, 192.

- Kathode Fall of Potential in Compound Gases; Relation to Fall in Elements** (Capstick), 356.
- Kew Observatory Committee, Report of the, for the year 1897**, 161.
- Krypton, New Constituent of Air** (Ramsay and Travers), 405; **Position among Elements** (Crookes), 408.
- Lang (William H.) On Apogamy and the Development of Sporangia upon Fern Prothalli**, 56.
- Larmor (Joseph) Note on the Complete Scheme of Electrodynamical Equations of a Moving Material Medium, and on Electrostriction**, 365.
- Light, Absorption in Dilute Solutions** (Edser), 374; **Extension of Maxwell's Theory of, to include Dispersion, Metallic Reflection, and Allied Phenomena** (Edser), 91, 374; **Relative Retardation of Components of a Stream of, when passed through a Crystalline Plate** (Walker), 79; **Velocity of, in Metals, Connection with Electric Conductivity of Metal** (Edser), 374.
- Lister (Arthur) elected**, 373; **admitted**, 412.
- Macallum (A. B.) On the Detection and Localisation of Phosphorus in Animal and Vegetable Tissues**, 467.
- McMahon (Lieut.-Gen. C. A.) elected**, 373; **admitted**, 412.
- Magnetic Deformation in Nickel, Influence of Temperature on Change of, with Time** (Jones), 44.
- Magnetic Field, Modifications of Spectra of Iron and other Substances produced by** (Preston), 26.
- Magnetic Properties of Iron, effects of heating on** (Roget), 258.
- Magnetic Range (Diurnal), and Period of Sunspot Frequency, relation between** (Ellis), 64.
- Magnetic Susceptibility of Liquid Oxygen** (Fleming and Dewar), 311; — — of Rocks in England and Ireland (Rücker and White), 460.
- Magnetism and Light; Zeeman Effect and Faraday Rotation of Polarisation Plane** (FitzGerald), 31.
- Marcet (William) A Calorimeter for the Human Body**, 232; — and Floris (R. B.) **An Experimental Inquiry into the Heat given out by the Human Body**, 242.
- Marine Life, Animal and Vegetable, relations between** (Vernon), 155.
- Martin (C. J.) and Cherry (T.) The Nature of the Antagonism between Toxins and Antitoxins**, 420.
- Maxwell (Sir Herbert Eustace) admitted**, 63.
- Meeting of March 3, 1898**, 1; **March 10**, 25; **March 17**, 63; **March 24**, 63; **March 31**, 205; **April 28**, 206; **May 5**, 212; **May 12**, 231; **May 26**, 270; **June 9**, 373; **June 16**, 412.
- Mendeléeff (D.) Experimental Investigations on the Oscillations of Balances**, 454.
- Metabolism in the Plant, Nature and Significance of** (Pfeffer), 93.
- Metargon, New Gas in Air** (Ramsay and Travers), 437.
- Monaco (Prince Albert I of) On the Meteorological Observatories of the Azores**, 206.
- Moore (John Carrick), Obituary Notice of**, xxix.
- Mueller, (Baron) Ferdinand von, Obituary Notice of**, xxxii.
- Murray (George) and Blackman (V. H.) A Study of the Phyto-Plankton of the Atlantic**, 269.
- Muscles, on the early Degenerative Changes in the Sensory End Organs of** (Batten), 61.

- Neon, New Gas in Air (Ramsay and Travers), 487.
- Nerve, Electromotive Phenomena investigated with Capillary Electrometer (Gotch and Burch), 300.
- Nerves, action of Sodium Chloride on (Gotch and Burch), 300.
- Newton (Hubert A.), Obituary Notice of, i.
- Nickel, Magnetic Deformation of, influence of Tension and Temperature on (Jones), 44.
- Obituary Notices of Fellows deceased:—Des Cloizeaux (Alf. L. O. Le Grand), xxv; Moore (John Carrick), xxix; Mueller (Ferdinand von), xxxii; Newton (Hubert A.), i; Quain (Sir Richard), vi; Sylvester (James Joseph), ix.
- Osler (William) elected, 373.
- Oxygen, Liquid, Magnetic Susceptibility of (Fleming and Dewar), 311.
- Papers read, lists of, 2, 25, 63, 206, 212, 231, 270, 373, 412.
- Parsons (Hon. C. A.) elected, 373; admitted, 412.
- Pearson (Karl) Mathematical Contributions to the Theory of Evolution. V. On the Reconstruction of the Stature of Prehistoric Races, 417.
- Petavel (J. E.) On the Heat dissipated by a Platinum Surface at High Temperatures, 403.
- Pfeffer (Wilhelm) admitted, 63; The Nature and Significance of Functional Metabolism in the Plant, 93.
- Phosphorus, Detection and Localisation of, in Animal and Vegetable Tissues (Macallum), 467.
- Photographic action of Organic Bodies, Metals, and Alloys on Sensitive Plate (Russell), 102.
- Phyto-plankton of the Atlantic (Murray and Blackman), 269.
- Pinus silvestris*, Fertilisation and Related Phenomena (Blackman), 400.
- Plant Energy, sources of (Pfeffer), 93.
- Platinum Surface, heat dissipated by (Petavel), 403.
- Polarisation, Rotation of Plane of, by Magnetism, connected with Zeeman effect (FitzGerald), 31; — Rotatory, of Electromagnetic Waves (Bose), 146.
- Porter (T. C.) Contributions to the Study of "Flicker," 347.
- Potential Energy, mutual, of Circular or Helical Current and uniform Coaxial Current Sheet (Jones), 192.
- Preston (Thomas) elected, 373; admitted, 412; On the Modifications of the Spectra of Iron and other Substances radiating in a strong Magnetic Field, 26.
- Protoplasm (Vegetable and Animal), Action of Anæsthetics on (Farmer and Waller), 213.
- Quain (Sir Richard), Obituary Notice, vi.
- Raisin (Catherine A.) On certain Structures formed in the Drying of a Fluid with Particles in Suspension, 217.
- Ramsay (William) and Travers (Morris W.) On a new Constituent of Atmospheric Air, 405; on the Companions of Argon, 487.
- Reid (Edward Waymouth) elected, 373; admitted, 412.
- Roberts-Austen (W. C.) On Surfusion in Metals and Alloys, 447.
- Rock Structures, Dendritic, &c., illustrated by drying up of Muddy Fluid (Raisin), 217.
- Roget (S. B.) Effects of prolonged Heating on the Magnetic Properties of Iron, 258.

- Röntgen Rays, source of, in Focus Tubes (Swinton), 432.
- Rücker (A. W.) and White (W. H.) On the Determination of the Magnetic Susceptibility of Rocks, 460.
- Russell (W. J.) Further Experiments on the Action exerted by certain Metals and other bodies on a Photographic Plate, 102.
- Schunck (C. A.) A photographic Investigation of the Absorption Spectra of Chlorophyll and its Derivatives in the Violet and Ultra-violet Region of the Spectrum, 389.
- Scott (Alexander) elected, 373; admitted, 412.
- Sensory End Organs of Muscles, observations on Early Degenerative Changes in (Batten), 61.
- Seward (Albert Charles) elected, 373; admitted, 412.
- Shenstone (William A.) elected, 373; admitted, 412.
- Snake venom—chemical nature of Antagonism of Toxins and Antitoxins (Martin and Cherry), 420.
- Sollas (W. J.) On the Intimate Structure of Crystals. Crystals of the Cubic System with Cubic Cleavage, Part I, 270; Part II, 286; Part III, 296.
- Sowter (R. J.) and Forsyth (R. W.) On Photographic Evidence of the Objective Reality of Combination Tones, 396.
- Specific Inductive Capacity, due to Motions of Electrically Polarised Molecules (Edser), 374.
- Spectra, Absorption, of Chlorophyll (Schunck), 389; — Influence of a strong Magnetic Field on (Preston), 26.
- Sphenodon, development of (Dendy), 440.
- Stature of Prehistoric Races, Reconstruction of (Pearson), 417.
- Stomata, Observations on (Darwin), 413.
- Stomodæum of *Xenia*, Gland Cells in (Ashworth), 443.
- Surfusion in Metals and Alloys (Roberts-Austen), 447.
- Swinton (A. A. C.) On the Source of the Röntgen Rays in Focus Tubes, 432.
- Sylvester (James Joseph), Obituary Notice of, ix.
- Taylor (Henry M.) elected, 373; admitted, 412.
- Teeth, Incisor, independence in young Sphenodon (Dendy), 440.
- Tomes (Charles S.), upon the Structure and Development of the Enamel of Elasmobranch Fishes, 54.
- Toxins and Antitoxins, Antagonism between, Chemical in Nature (Martin and Cherry), 420.
- Travers (Morris W.) and Ramsay (William). On a new Constituent of Atmospheric Air, 405; on the Companions of Argon 437.
- Tuatara, Development of (Dendy), 440.
- Tutton (A. E.) A Compensated Interference Dilatometer, 208.
- Vaporisation of Metals and other bodies indicated by Photographic Action on Sensitive Plate (Russell), 102.
- Vernon (H. M.) The Relations between Marine Animal and Vegetable Life, 155; the Relations between the Hybrid and Parent Forms of Echinoid Larvæ, 228.
- Walker (James) On the Relative Retardation between the Components of a Stream of Light produced by the Passage of the Stream through a Crystalline Plate cut in any Direction with respect to the Faces of the Crystal, 79.

- Waller (A. D.) and Farmer (J. B.) Observations on the Action of Anæsthetics on Vegetable and Animal Protoplasm, 213.
 Wave-slowness, Hamilton's surface of (Walker), 79.
 White (W. H.) and Rücker (A. W.) On the Determination of the Magnetic Susceptibility of Rocks, 460.
 Whittaker (E. T.) On the Connection of Algebraic Functions with Automorphic Functions, 267.
 Wilson (E.) Aluminium as an Electrode in Cells for direct and alternate Currents, 328.
 Wimshurst (James) elected, 373; admitted, 412.
Xenia, Gland Cells in Stomata, Pseudopodia in Endoderm (Ashworth), 443.

ERRATUM.

Page 206, line 8. For "Professor Clifton" read "Captain Abney."

END OF THE SIXTY-THIRD VOLUME.

20

1

1

To avoid fine, this book should be returned on
or before the date last stamped below

--	--	--

PHYSICS - MATH

508
R88 P

U.63

